RESEARCH IN MEDICINE

AND OTHER ADDRESSES

SIR THOMAS LEWIS

LONDON
H. K. LEWIS & Co. Ltd.

RESEARCH IN MEDICINE

RESEARCH IN MEDICINE

AND OTHER ADDRESSES

BY

SIR THOMAS LEWIS

C.B.E., F.R.S., F.R.S.E., M.D., D.Sc., LL.D., F.R.C.P.

PHYSICIAN IN CHARGE OF DEPARTMENT OF CLINICAL RESEARCH, UNIVERSITY COLLEGE HOSPITAL, LONDON; HONORARY CONSULTING PHYSICIAN TO THE MINISTRY OF PENSIONS; CONSULTING PHYSICIAN, CITY OF LONDON HOSPITAL; FELLOW OF UNIVERSITY COLLEGE, LONDON

LONDON

H. K. LEWIS & Co. Ltd.

BY THE SAME AUTHOR

CLINICAL DISORDERS OF THE HEART BEAT

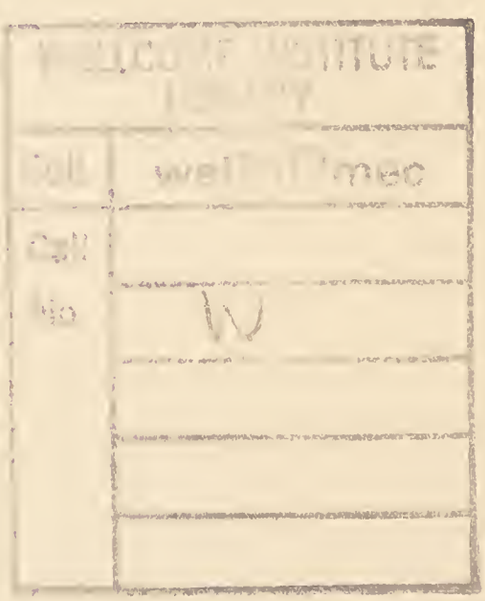
CLINICAL ELECTROCARDIOGRAPHY

DISEASES OF THE HEART, DESCRIBED FOR PRACTITIONERS AND STUDENTS

CLINICAL SCIENCE, ILLUSTRATED BY PERSONAL EXPERIENCES

VASCULAR DISORDERS OF THE LIMBS, DESCRIBED FOR PRACTITIONERS AND STUDENTS

ETC.



PREFACE

THROUGHOUT the years of studentship it was my fortune to come under the influence of men whose outlook was original inquiry. Swale Vincent at Cardiff and Starling in London were physiologists; Horsley, under whom I held my first resident appointment, used the same experimental method. From Vincent I learned to be critical, and he gave me my first opportunity to investigate. Starling and Horsley were tremendous workers; both were inspiring men, of outstanding intellect and originality. I name these three because they first interested me in research, and brought me the profitable associations with British physiology that I have since enjoyed. Because of these associations there was no period of my student career when I was not working upon some physiological problem. And this work led me towards the belief that the problems daily encountered in the wards of our hospitals are capable, despite their complexity, of solution by a similar form of investigation directly applied to man. But it is to be confessed that I wavered in this belief from time to time, because on the one hand my physiological friends were convinced of the unrivalled efficacy of animal experimentation, and because I witnessed no successful investigation of the mechanism of disease carried out on man. Horsley's investigations at that time were mainly conducted in the laboratory. It was from James Mackenzie, and from the intimate view I was privileged to obtain of his work, which he was still doing with world-famous success, that I won confidence in the fundamental value of the direct or clinical method of approach; for all Mackenzie's work was done on man, and its influence was profound. When the nature of complete irregularity of the human heart was discovered, very largely by clinical investigation, this confidence was justified and a clearer evaluation of the relative positions of animal experimentation,

which also played an important but not predominant part in the proof, and of clinical experimentation became possible.

There grew in my mind out of these early associations and experiences two general guiding motives. Physiology in the middle of the nineteenth century was subservient to medicine, but its ardent votaries won leisure to build up physiological knowledge; they built so successfully that they broke away from medicine and gained independence and academic freedom. From that time British physiology raced ahead and soon outstripped medicine in scientific standing.

My first general motive arose out of the hope that a science of medicine itself might similarly develop, through the freeing of investigators from the necessity of earning livelihood by practice, and through whole-hearted devotion to problems touching immediately upon human disease. For years this aspiration was unsatisfied; there was so late as 1914 no single post in this country for full-time work other than the temporary work offered by an occasional scholarship; there was absolutely no prospect of a research career. In the spring of that year, as the pressure forcing all young physicians along the road to practice weighed upon me, I wrote anonymously a plea to a daily paper.* Re-reading it to-day, I find therein expressed an ideal that I have since consistently pursued, and barriers to its achievement the nature of which I have since tried more clearly to analyse and expose. Some of the latter have disappeared, others are disappearing. We have come a good distance since those days.

The first great move came with the establishment of the Medical Research Committee (afterwards Council) in 1914, and with the allotment of State funds for medical research. It was inevitable that Clinical Science would soon share in the opportunities thus afforded. The recognition by the Council of the clinical need has not decreased with time. The separate creation by the Board of Education of full-time professorial units in the immediate post-war period gave medicine potentially

---

* *Morning Post*, April 25th, 1914.

and actually another great forward impetus. The movement is nourished now from the great benefactions of Beit, Foulerton, Rockefeller, and Nuffield. But by no means the least factor making for progress has been a steadily widening recognition that Clinical Science, besides using for its purposes knowledge derived from physiology and other kindred sciences, has itself a fundamental contribution to make to medical science as a whole. The inescapable truth is that to cure or to alleviate human disease research must begin and end in direct studies of sick people. The resulting belief that Clinical Science is the vital medical science has been deliberately cultivated, and has become the chief tenet of a conscious movement, growing in vigour and strength through the recruitment and achievements of able young workers.

There was a second motive. While desiring to see Clinical Science grow to a strength comparable to that of physiology, I looked still for guidance to the science to which I had first given homage, and hoped then as now that the bonds between the two would strengthen and that the two would move hand in hand as partners in a complete team. There is no doubt in my mind that physiology is the finest early training for a student of human pathology, and that a knowledge of normal functions is always fundamental to an understanding of the manifestations of disease. Yet I cannot accept the beliefs, which Claude Bernard expressed many years ago and which some still hold to-day, that medicine is but applied physiology, and that the reproduction of human disease in animals can be used successfully as a general method of study. It is palpably clear that physiology, alone and unaided, would be sterile for medicine; it is the union of all branches of medical science which gives medical investigation irresistible strength to proceed.

Those whom the foregoing brief personal narrative interests will find the two motives running as entwined threads through the several public utterances here republished; they will understand the stress I have laid upon the teaching of human physiology to students of medicine; they will perceive no inconsistency in the frequent championship of animal experimentation which in

the post-war years I felt called upon to undertake; they will realize that the advocacy of clinical research as vital to medical progress implies no derogation of physiological research or physiological method, and they will recognize such advocacy as a proper stimulant to the alignment of forces that should act in concert. Because it has fallen to me to be a chief exponent, the following papers will be found to indicate the position as it was, to state as I have seen them the principles underlying the new endeavour, and to mark in parentheses many of the forward steps that have been taken in recent times. The scattered papers have not been very accessible to those interested, and so I have responded to suggestions that I should republish them under one cover.

RESEARCH IN MEDICINE
AND OTHER ADDRESSES

AN ADDRESS ON THE RELATION OF PHYSIOLOGY TO MEDICINE*

The relations between the science of physiology, which studies the normal functions of the animal body, and medicine, which studies and treats the human body in disease, are so intimate and manifold that the subject before us may be approached from many different points of view. When you, Mr. President, asked me if I would open this discussion, I felt some hesitation in accepting, knowing that I should speak from the standpoint of the physician. Although I have from time to time strayed into the proper realm of physiology, nevertheless I would ask you to remember while I address you that I am primarily a physician. It is for that reason that this paper emphasizes, not the standpoint of physiology, but the standpoint of medicine, for it is the welfare of medicine which I have firstly at heart.

Because the dependence, now, in the past, and in the future, of medicine upon physiology is not dwelt upon; because I do not speak, as I could speak, of the great and lasting benefits which physiology has conferred, and continues to confer, upon medicine as a science, and upon medicine as a healing art, it is to be hoped you will not think on this account that I am insufficiently alive to this relation of the two subjects. It could serve no useful purpose to come to physiologists and tell you what you all know, what is to every thinking man apparent—namely, that any knowledge which has reference to the normal functions of the animal body, however remote from practice that knowledge

---

* Delivered in opening a discussion at the Physiological Section of the British Association at Cardiff, August 26th, 1920; and first published in the *British Medical Journal*, September 25th, 1920. Slightly abridged.

may seem at the time of its discovery, is sooner or later reflected or incorporated in medical thought and medical practice. Physiology, originally the offspring of medicine, has become to medicine in its old age a chief pillar of support. But, while this filial and noble aim of physiology has fulfilled, and continues to fulfil, its great purpose in nourishing and supporting its sire, it is fully recognized that physiology has other and equally noble aspirations. It seeks knowledge for its own sake, for the enlightenment of thought in other spheres. To take the view that physiology exists as the servant of medicine were as narrow as to countenance and encourage a complete servitude of human offspring to their parents. Each of these offspring may rightly claim a large measure of liberty. But as my own life interest is in medicine, so I may be forgiven if I rank myself with those who place the filial function first; I may be forgiven if, in recognizing that the two sciences, physiology and medicine, have mutual obligations to each other, I place, by reason of the same prejudice, the obligations of physiology well to the fore.

The obligations of physiology to medicine are twofold: to teach the student bent on the practice of medicine the normal functions of the human body, and in doing so to train him as an accurate observer and thinker; and to lay a foundation upon which the house of medical science shall erect itself. If you will, you may weld the two together and say that in its relation to medicine the function of physiology is to gather, classify, and spread knowledge and methods of studying the normal functions of the animal body. The first statement suits my purpose best, for it allows me to divide my thoughts into two chief channels, the teaching of medical students and the direction of physiological research—two channels, however, which should not diverge too far from one another. From both points of view the very closest association of physiology and medicine is necessary. The underlying thought has been to try and find means by which these sciences might be brought to closer and closer union.

Let us come at once to the pith of what I wish to say in regard to the teaching of physiology to medical students. In many respects it is essentially a controversial subject, and all the more suited therefore for discussion. The views expressed are personal views. It is my personal conviction that the pivot around which physiological teaching should play is human physiology; that human as opposed to animal physiology should form the central point of physiological teaching to medical men. That

the physiological lessons are to confine themselves to the human body is not by any means what is advocated; such a course is clearly impossible, while by far the largest part of our knowledge of the functions of the human body is still derived analogically from observations upon the lower animals. The standpoint is that knowledge which has been obtained or is now obtainable by observation upon man himself should be constantly stressed, and that animal physiology should be taught where it supplements knowledge which is so obtained. It is of considerably greater consequence to the student of medicine that he should witness a given and normal phenomenon, or gather a given principle, by observation upon the human rather than upon the animal body; when, therefore, a phenomenon can be displayed to him both in the man and in the beast with equal accuracy, it should be displayed to him as it occurs in the former. When it cannot be displayed in man, or when it cannot be displayed with the same fine degree of certainty, then animal physiology should be called in. I would go further, and say that when a phenomenon can be displayed in man, even though it can be displayed less accurately than it can on the animal body, it should be demonstrated and stressed, not to the exclusion of the more precise demonstration on the lower vertebrate, but alongside of it, so that the two demonstrations may be evaluated and the two compared. There are several vital reasons why this should be the trend of modern physiological teaching to medical students. First and foremost is this reason: *that students in their most impressionable period would thus learn how to investigate the human body.* Not infrequently it is discovered in talking with physiologists that they but dimly realize the difficulties of this task, fail fully to appreciate how enormous is that barrier between physicians and the working organs—namely, the unbroken skin. Yet that barrier must and can be passed; the passage is vital to the present and future success of medical science and practice. The problem of what is happening in the human body is infinitely harder to solve than that which asks what is happening in the animal body, for in the former case our methods of examining and of observing are usually far more indirect. The student is taught to stimulate the vagus nerve in the frog and to note the effects; at the same time he should be taught to press upon the vagus in* the carotid sheath of his fellow student, and to observe

---

* The words "the vagus in" should now be omitted.

the consequent slowing of the heart beat. Physiologists have to face the fact that the large majority of the students who pass through their hands will encounter in their daily work problems far harder to solve than those with which they themselves deal in their laboratories. I speak from experience in saying that medical students, when they come to us in the teaching hospitals from the physiological laboratories, come to us insufficiently equipped in this respect. They have not yet learnt to handle a man; their senses of hearing, and especially those of touch, are insufficiently developed; yet they are to handle men for the rest of their days; and my plea is that, with very few exceptions, they never will learn efficiently if they do not begin to learn while they work under your direction. They come to us in the hospitals after practising for two or more years relatively direct and conclusive methods; they come, many of them, with a rich knowledge of the functions of the body, with clear ideas of how problems of the animal body are approached and solved; they soon find by experience—often bitter experience—that most of these relatively exact methods which you employ are inapplicable to the study of living men; that many which are applicable are by their specialization soon beyond their reach; that far less direct, far cruder tests are of necessity used; that inferences are based upon premisses of a totally different quality. At the same time they are brought face to face with a mass of detailed knowledge of a new kind, they are asked to accept a new series of conclusions; upon many of these conclusions, it is true, their past teaching will bear; with many more their physiological teaching will not directly connect. The arduous work of the medical student in his final years is known to you; no one can pass through this last course, sifting and weighing a fraction of what is taught him. The great bulk of what he hears in his hospital years must be taken on trust. It is by the force of these circumstances that the medical student invariably becomes empirical; and the measure of his empiricism is the measure in which he forgets his physiological teaching; this teaching becomes buried more or less in a mass of new experiences and methods. My contention is that his physiological training would not be lost to him in the large measure in which it indubitably is lost were the shock of change less, were his old experiences more in harmony with the new. It has been an intense interest of my own to watch for not a few years this stage of the medical student's career; it is his most critical time. I have closely

watched a goodly number of men unusually well trained in physiology—I refer to men who have spent more time in physiology than the curriculum dictates—and have yet to see an example in which the deadening influence of this transition has not been conspicuously displayed upon the faculties of criticism and inference. It is an influence that I am conscious, from personal experience, that I have not escaped; it takes years to outlive it. You train a man to weigh with a nicely adjusted chemical balance; when you start him on his life's work a crude scale is alone available to him, and from the moment it comes into his hands he is set to weigh hard. He weighs and he forgets the old balance; in general he never fully appreciates the defects and limitations of the new; in time he acquires a mistaken confidence. These limitations, these defects, he should surely know beforehand; familiarity with those tools which are to be his as a workman, exact knowledge of their capacity to help him or to lead him astray, should be his from the very start. To train a man fully in the use of delicate instruments, to bring him then to his first real workshop and to set him to his bench of coarse and unknown tools, is to inflict upon him a thing which to many brings apathy, to some revulsion. Let him see and use his delicate instruments by all means, but let him clearly understand that these are in his hands, not only for the knowledge they bring, but also for comparison with those more simple devices which are also there for him to see and use, and which are to be eventually the weapons with which he will fight disease. In that way, and in that way only, so it seems to me, will he lastingly appreciate the limitations which circumstance and method impose upon him, and in that way will he assess the true values of his method. Such is, largely in metaphorical language, what I have in mind in pleading for human physiology. It will be said, perhaps, that the fault lies in medical teaching; in great part that is true, but it is not to be forgotten that medical science labours before unusual obstacles; neither is it to be forgotten that teachers of medicine have themselves all passed through this same ordeal—an ordeal which leaves none unscathed; medical practice, driven by necessity, seizes those weapons which are alone at hand; the old trapper has a too easy-going confidence in the gun which has long been his only available companion. The confidence displayed by medical men in their methods is a chief fault in medical practice to-day; it is a personal opinion that that confidence can be modified, most materially and most

quickly, in the physiological laboratory, by training students how to examine the human body and teaching them what physiological conclusions they may legitimately draw from such investigations; to show them side by side with such methods of investigation what can be done when, as with the lower animals, greater facilities are presented to them; to supplement conclusions drawn from experiment on other animals. An arrangement seems desirable whereby clinical material, illustrating clean experiments of disease which throw light on normal functions, should be brought into the hands of physiologists and their students. Such co-operation would form a valuable connecting link between physiology and clinical medicine, stimulating research and urging teaching in the direction which it should take—physiological teaching becoming more human, and medical teaching more in consonance with physiological thought. Such cases would illustrate instances of simple lesions of the nervous or cardio-vascular system or special senses. I have in mind clean transections or ablations and their effects which, when shown side by side with corresponding evidences from animal physiology, would form most impressive demonstrations. Demonstrations upon the human subjects are always impressive. Few physiological students who have heard the story forget Alexis St. Martin. None would forget if they saw him in life. Salivary, gastric, or biliary fistulae are not so uncommon that they might not from time to time be shown to physiological students. Many of the patients who visit our hospitals because they present anomalies of the ductless glands might also enter your laboratories. I look forward to a time when all physiological laboratories which train medical students may be equipped with those instruments of examination which are to be found in teaching hospitals, for there are very few of these instruments which will not display in some measure the normal functions of organs of the human body. As an example I may cite the movements of the alimentary canal as these are shown by the x-ray tube.

But in regard to such instruments and devices I have some particular remarks to make.

There is, if I mistake not, a prevalent opinion that the medical man of the future is to be equipped as a physicist, as a chemist; that he is to have behind him an array of delicate apparatus and reagents with which exact measurement is to be made possible to him; and that in these directions the salvation of

medicine is to be sought. If I am correct in my belief that such is a current view, then I would state emphatically that I believe it to be a profoundly erroneous view. It confuses two issues—the daily routine of the practitioner and the work of the special investigator. These stand apart: they always will stand apart. You cannot deal with masses of patients by using refined methods; repeatedly it has been tried; it always breaks down. The chief weapons of the general practitioner are to-day, and will remain, his own unaided senses; these are supplemented by a few simple devices. The healthiest trend of modern medicine—and let it be said at once that it is a trend which is anything but universal—is to dispense as far as possible with special methods of investigation in dealing with the everyday patient. The special method is for the investigator into pathological processes; it is not for the pure clinician. The use of special devices, of special tests, almost always leads one way: the devices and tests are employed by men who cannot have the training, cannot afford the time fully to understand, and a method imperfectly understood is, generally speaking, worse than useless. Let the knowledge of disease be built up by means of such instruments; let us sift from this knowledge what is of immediate and unquestionable service to the practice of medicine; let us then find how the special test may be supplanted, by methods perhaps not quite so accurate, but methods which can be at once and universally employed. Believe me, you will never see the day when the medical practitioner will rightly interpret delicate records of the heart beat; but you will see the day when he will know the chief things which those records have taught us, and how he may utilize that knowledge in his daily work. A medical practitioner cannot hold a working knowledge of these refined methods; each has its growing complexity, each formulates innumerable rules and reservations; each is to him an abstruse method, applicable to but a small fraction of his patients. The plea for him is simplicity, not bewildering complexity. Treat polygraphy, treat electro-cardiography as sources of knowledge and of generalization, and you may say that these methods have potentially fulfilled their great purpose; they have greatly simplified the diagnosis and treatment of heart disease; but treat them as parts of the medical man's equipment and you will load further what is already overloaded. It is eight years since, in the preface to my small book on *Clinical Disorders of the Heart*—a book describing how

varieties of disordered heart action may be recognized by simple tests—I first expressed these ideas; they have since grown towards convictions, and, as you are aware, have found a powerful advocate in Sir James Mackenzie. Medical practice will grow more scientific as it becomes more and more simple—as the practitioner grasps great and fundamental truths in their relation to common diseases; it will not grow scientific in proportion to the mass of detail—impressive enough, maybe—which he is able to pack into the compass of his brains. The place of these refined methods is the laboratory and the teaching hospital. In the teaching hospital they are there so that the student may learn how knowledge is acquired. I have never, as a teacher of medicine, taught these methods to undergraduate students; neither will I do so. It is for them to use their fingers and their sight, but the instruments are there to check such observations, to show what can, and equally what cannot, be done; they are there so that the student may see how the knowledge which he is to use is won. We have to teach our students not only how to handle disease, but also upon what the knowledge we impart is founded.

The second reason which is urged for a greater concentration upon human physiology in teaching is of a different, though hardly less important, character. Medical men are insufficiently conversant with what constitutes normality, or the range of normality, in the human being. They regard as pathological many phenomena which are in reality not only compatible with health, but are actually physiological. It is of much consequence that this argument should be brought home, and it may be done by means of a few notable examples. I take these from that sphere with which I am most fully acquainted by personal experience—the sphere which studies the circulation. They could be multiplied, and many others of the same kind could be cited from other branches of study, but these will suffice. Take a very simple case: Stroke the back of a normal healthy young subject and watch the vascular reaction in the skin; the line pressed and the surrounding skin displays after a while a red line; maybe it soon becomes surrounded by an area of blanching, maybe a line of blanching alone appears. These reactions, perfectly healthy reactions as they are, have been used, are still used, and will be used for many years to come, as important signs of particular and serious disease. Take the effect of respiration upon the human pulse. If the

pulse falls away in strength in inspiration—*a fortiori*, if it disappears—it is held to be a sign of serious mischief in the pericardium, or of some other grave disorder; actually both signs are compatible with—nay, are common in—perfect health; the normal waxing and waning of the pulse rate in natural breathing has been responsible, and is still responsible, for the enforced invalidism of hundreds of young children. Normal variations in the amplitude of heart sounds, the presence of those murmurs which almost always accompany an accelerated action of the healthy young man's heart, send hundreds to their beds or couches annually; the same normal phenomena kept very many hundreds of strong, able-bodied men from their proper stations in the time of the Great War. These and similar variations in normality are insufficiently taught in our physiological schools, where they can be most convincingly taught. Any departures from a single physiological standard come inevitably to be regarded as abnormal; busy medical men have no time to seek for and to study controls; for the most part they leave the physiological study of man when they leave the physiological laboratory and lecture theatre. The result is that the knowledge of normality in man is often far more elementary than it has business to be.

Take the physiology of exercise, the measured amount of work which healthy men of different ages and of different training can do in a given time, or the reactions of pulse and respiration which a simple test exercise should call forth in his body. That simple physiological knowledge is vital to proper medical practice, yet only a few medical men have this requisite knowledge to-day. That surely is a reflection on the method, if not of physiological then of our combined teaching. It is sad to think that medical men should fail to recognize a sick man, or should call a healthy man sick, because they lack full knowledge of healthy reactions; yet these are common daily blunders. When I look back over the work upon which I have been engaged in the last eighteen years since I left the physiological laboratory I discover that a large fraction of my time and energy has gone in discovering phenomena of health which hitherto I had learned to regard as manifestations of disease. To teach the principles of their science, to teach men to observe accurately and to think critically are wise aspirations of physiologists. A man may be given a scientific training in chemistry, but if practical chemistry is to occupy little or no part of his active life, much

of the virtue of the training goes out of it. The argument—it is no new argument—is that physiology as it is now taught to medical students is insufficiently correlated to medical practice: it is not human enough. If, in illustrating a principle or in impressing accuracy of observation, there is a choice of subject—and selection has become imperative with the rapid expansion of your science—it is better to choose an illustration which may be followed by a corresponding demonstration upon man. Do medical men show unusual discrimination when they come to weigh the pros and cons of a new problem ? Are their explanations weighed in the critical spirit which their physiological teachers have tried to instil into them ? Are they unusually acute in assessing the value of evidence ? I do not answer these questions negatively, but lack the assurance to answer them affirmatively. If they are to be answered negatively, does it not mean that there is some weakness in physiological teaching ? for physiology is the chief philosophical subject which they study. And may not this defect, if it exists, in part at least result because they learn to philosophize upon questions which are soon forgotten in the daily routine of practice ? It matters little to a practitioner whether Ludwig or Heidenhain was in the right; he does not base his treatment on one or other view. An account of many such controversies, and especially those which have led as yet to no final conclusion, might well be replaced by an historical account of those problems which are more vital to the daily needs of medicine, and these problems might equally illustrate processes of argument and thought. With purely physiological hypotheses, we, as pathological investigators, have little or no business. I use the term hypothesis in its true meaning—an assumption intended as a temporary explanation of observations, and put forward in the hope that with extended investigation such hypothesis may harden into conclusion. A published hypothesis is usually in essence a redoubt built by its conceiver; he and his party labour to strengthen it; but its very nature invites, and sooner or later provokes, an attack, and, as with redoubts, it is but rarely impregnable. The attack and counter-attack may be and often are prolific of discovery, whether the redoubt holds or falls. It is at once conceded that physiological hypothesis is essential to the physiologist; it has not the same value to medicine, it is often dangerous when introduced to medicine. The observations and conclusions of physiology are indispensable to us; but too often, from the

inability to weigh evidence, hypothesis and conclusions are confused, and the former becomes the basis of pathological speculation. This is a widespread and regrettable habit amongst pathologists and clinicians. The pathologist or clinician who founds his own assumptions on the assumptions of physiology builds to see the complete collapse of his edifice. He who founds his assumptions on the observations and conclusions of physiology may still see his house totter, but the foundation will remain secure, and, happily, some parts of the superstructure may remain intact if solidly laid.

In turning more particularly to questions of original research, I again stress the need of more closely uniting physiological and pathological thought; in that way only will the student of medical science remain conversant with physiological conclusion, and be in a position to divide it sharply from hypothesis. While it would be good to see our anatomists in our *post-mortem* rooms, it would equally be good to see our physiologists more often in our wards. It is scarcely to be doubted that such visits would lead to very fruitful investigations, would correct misconceptions on both sides. That is but one way in which constant touch might be attained. In the " Physiological Society " we have possessed in the past a connecting link, though it be but an occasional link; to this society the leaders of pathological research, many leaders of clinical research, have belonged; in times past they have provoked some of your most interesting and fruitful discussions, stimulated or produced many important physiological researches. But as physiology becomes ever purer (more physical and more chemical) it becomes more and more difficult for men engaged in clinical investigation to feel at home at these meetings. It is no answer to say that medicine should follow the same lines, go back to the simple physical and chemical basis; it cannot do so at the same rate; medicine is inevitably preoccupied with end problems, and cannot escape from the expedients and opportunisms which its constant association with very sick people forces upon it. Here arises a tendency for the pure physiologist and purely clinical investigator to drift apart. They must be kept together, not only for the sake of medicine, but also for that of physiology. The breach has been partly closed by contact at the meetings of the admirable society to which I have referred; it has also been closed in part by a group of men who have devoted much of their energy to this task. I refer to those investigators who have devoted them-

selves to what is now often termed " experimental medicine." Taking for their field of study diseases of particular organs or systems of organs, they have sought to bring the physiological and pathological side of their subject into harmony. Horsley, Ferrier, Head, Garrod, Mackenzie, and many others have been recently or are still amongst this group of active workers. The group of men has been formed from those practising medicine or surgery, notably from the ranks of neurologists; students of applied physiology have been drawn too exclusively from the ranks of those who are primarily clinicians, and insufficiently, as I think, from the ranks of trained physiologists. A chief reason why this has been so in the past is that " experimental medicine," as it is often called, has offered no immediate chance of livelihood. My view is that the number of physiologists who by contact with clinical problems are inspired to work upon problems of applied physiology is still insufficient; the war period, during which many physiologists were so engaged, showed the fruitfulness of such contact. Medical research is now nourished on a new basis in this country, and it looks as if the link, " experimental medicine," may, when it is fully forged under the Medical Research Council, go far to fill what is still an obvious want—namely, the closer co-operation of all research work in the clinical and pathological fields with contemporary physiological thought.

OBSERVATIONS ON RESEARCH IN MEDICINE: ITS POSITION AND ITS NEEDS[1]*

With the greatest activity of medicine, that which ministers to the immediate needs of sick individuals, and which may be called *curative medicine*, we have here no direct concern. Our present interest is directed to that activity the immediate purpose of which is to advance knowledge; and this for convenience will be spoken of as *progressive medicine*. Progress in medicine can be followed historically along two main lines. The first is that traced from Hippocrates through Sydenham; it is the method of observation, and it has given us our descriptions of disease. The capacity of clinical observation to separate and define diseases was greatly enforced by the work of the famous morbid anatomists; the combined observations laid the foundations of modern clinical medicine. Laennec and Skoda, exponents of tested methods of physical examination; Bright, Hodgkin, Corrigan, Addison, who so ably identified and described

[1] For twenty-five years engaged almost wholly in research work upon problems arising directly or indirectly out of contact with patients; for many of the earlier years verging upon, or partly in, consulting practice, I have had what is perhaps a unique first-hand experience of the ways of medical research within the walls of a teaching hospital, from the time when there were no facilities or aids worth the name, to more recent times when these have come. Out of this experience, and out of an unusual association with younger workers, there should be something that can usefully be said to help these on their way. What is said is not put down hastily, but is the outcome of long years of experience, thought, and discussion; the views are in fact developed from those expressed (*Morning Post*, April 25th, 1914) by myself sixteen years ago.

I would be deeply concerned if anything written in this article should give rise to the impression that it has been inspired by any personal difficulties in obtaining facilities for work in the past, other than those difficulties that have been inherent in the situation; on the contrary, I recognize and gratefully acknowledge that my work in past years could never have been carried out without the abundant good will and help of my hospital colleagues.

---

* First published in the *British Medical Journal*, March 15th, 1930.

individual diseases—these names but exemplify that most brilliant period of clinical activity, which was at its height in the earlier years of last century.

The second line of progress has been by the experimental method, using this word in its broadest sense; it is exemplified in the physiological field by the work of the physician Harvey and by Claude Bernard; in the bacteriological field by Pasteur and Koch.

While the methods of pure observation have long since passed their period of greatest activity, the experimental method continues to display amazing progress and growth. The experimental method in its development and in its deeper searchings has found the use of the lower animals increasingly necessary to its study of health and disease, and has led to the foundation of physiology and pathology as independent sciences, and to the great growth of modern laboratories; thus progress in medicine has come to be identified more and more with the work of these independent sciences and with the laboratories. Research, and especially experimental research—using the term "experimental" as it will be used throughout to designate experiment and not necessarily experiment on animals—tends to be delegated more and more to the laboratories, while clinical investigators have been brought by circumstances to become again content with observation, the ancient, traditional method of medicine. Cleavage along these lines is distinct and is deplorable, since it tends also to split off clinical medicine from invaluable contacts with physiology and pathology, and to render these last too exclusively intent upon their own spheres of work.

The final and practical use of almost all medical science is to maintain health and to combat disease in mankind. It is in the very nature of things that the study of disease, to be effective, must begin as it must end, with disease itself, and that all knowledge applicable to human disease must owe its inspiration, directly or indirectly, to intimate contact with disease as this exists in living man. If the vitality of medicine is to be increased, it must be clearly recognized that there is a fertile science that deals primarily with patients, and this must be encouraged to a more vigorous growth. The science might be called "experimental medicine" if this term had not come to convey too strongly the idea of experiment on animals. It would be called clinical pathology if the latter were not now fenced around by test tube and needle. It may be termed

Clinical Science. This science seeks, by observation and otherwise, to define diseases as these occur in man; it attempts to understand these diseases and their many manifestations, and here especially makes frequent use of the experimental method. It makes definite experiments upon disease or watches the effects of experiment conducted by injuries, however these arise; it culls, or actually creates, and uses physiological and pathological knowledge immediately related and applicable to the diseases studied. Its value has been abundantly and frequently displayed in this country by such experimental clinicians as Ferrier, Horsley, Mackenzie, and Head. The very mention of these names is in effect a definition of the science that is in mind. Their work was not work that could be delegated to laboratories; it was inspired and sustained by direct contacts with disease; it was carried through in very large or chief measure by observations on sick people. The chief motive of the present paper is to inquire how this Clinical Science now stands. Admit that, like other sciences, it can be fostered; admit that, as with these, its development may not be left solely to sporadic enterprise, but can be hastened and guided by the creation of opportunity, and it is not hard to show that it now suffers from neglect.

We may begin to display this neglect by considering the present training and opportunities of the young worker, who, having completed his training in the preliminary sciences, aspires to clinical research. The first part of his course is set. He must become competent to manage sick people. In the past for two, in the future for three, valuable years—and while a contemporary student seeking a career in physiology is finally preparing for, or is actually engaged in, first researches—his attention is largely focused upon fitting himself to recognize a multitude of types of patients, and to tend their immediate needs. Qualification, at the least, is, and must continue to be, inevitable. Under our present-day system qualification is, however, but a preliminary; it is the later stages of preparation, through which the student must now pass inevitably, or almost inevitably, that require our very careful and serious consideration. The governing fact is that no student after qualification can expect to obtain control of clinical material, and at the same time to enjoy freedom to work upon it under adequate facilities, unless he enters upon and completes a well-defined

career in a general teaching hospital. This career is of a conservative and most exacting type; it comprises not only house appointments with their valuable and early experiences of responsibility in handling disease, but a series of higher examinations, junior appointments, and heavy routine work, extending over years and arranged from one standpoint—namely, the training of a consulting physician. The man, ardent for research, who enters upon this course begins a long period of intense work, drawing him continually to its natural goal—consulting medicine; for his researches there is no certain prospect, and from this standpoint the ending is almost always barren. The reason is that preparation for success as a consultant and preparation truly suited to a career of clinical research present irreconcilable and deep-seated differences. Because the man who aspires to do research has at present no real option, but must pursue the first form of preparation, whether he desires ultimately to embark upon consulting work or not, it becomes imperative that we should examine this system and point out its defects in so far as it adversely affects a research career. And let it be said that on the matter of the suitability of our hospital system of training as a preparation for curative medicine no opinion is here offered other than to state that in many respects it is admirable for the purpose for which it has been devised; it is, however, a distinct question from the one now considered. The opinion that is put forward is that this system, and the atmosphere surrounding it, is but ill suited to the advancement of medicine by research, and reasons for this view will be considered at some length.

An obvious defect in present training is the natural emphasis laid upon diagnosis. Diagnosis makes intense and incessant demands upon a man's time and energy over the early years of his studies. All who frequent the wards of general teaching hospitals realize how dominant is its note and recognize the constant urge of their surroundings towards it. Diagnostic knowledge and ability is the first essential of good practice; it therefore becomes in teaching hospitals the most direct and universal measure of medical learning; it is the stepping-stone to personal success. Its highly competitive character is mainly responsible for the atmosphere of intenseness in these institutions. It demands intimate and long experience of all kinds of diseases and of the manifold variations and incidents that each may display. But, from our present standpoint, it lends

an exaggerated importance to knowledge of numerous rare diseases, of newly described phenomena of minor interest, and of newly described methods and tests. A desire for encyclopaedic knowledge is fostered and grows; it moves in the direction of detail, for the phenomena studied are of great complexity and diversity, and present few of those connecting links in the form of principle or generalization that would serve to lighten what otherwise becomes a feat of giant memory. The result, and the quick result, of this system is the loss of much that is of value in the earlier training. By unavoidable neglect, the physics and the chemistry of the earlier student days pass into oblivion or become so rusty as to be of no more than occasional service. Even physiology, more recent in the recollection and offering easier points of contact, suffers very seriously; so that few can maintain more than a little part of their learning in it, and none can hope in these years to keep abreast of its continued advance. The physician at an early stage can and does throw much or all of this knowledge to the winds; there is knowledge of far more value to his daily work, and this he assiduously and rightly gathers in.

Again, in the attempt at clinical omniscience, learning drifts into superficialities and is not profound, for there is little time for critical weighing, for sorting, or comparison, and therefore there is little correlation of observation and simplification of ideas. Intensive clinical training, as this exists, leads in general to no real grasp of the problems set by disease or of means of solving them; it is too purely observational for this.

Between the method and outlook of curative medicine on the one hand and of progressive medicine on the other there are definite, inevitable, and sometimes profound divergences. Consider diagnosis again. A diagnostician goes into his wards with one chief goal in view—it is to place a label correctly, to say that here is a species or a subspecies that someone has described, its name being this or that. It is legitimate to define diagnosis from our present standpoint as an attempt to recognize the known. That is an end-point.

When the clinical researcher goes to his wards his purpose is not to recognize the known, but to face the unknown. Diagnosis to him is but a starting-point, and when made it must be absolute. A diagnosis that is not reached without demonstrable security is unpermissible. Thus, while the interminable daily

discussions as to what malady this or that patient suffers form a vital part of the consultant's training and of his work, they possess a very limited value to the research worker. And here it is to be repeated that he who would work with proper facilities in clinical research, works perforce at the present time in institutions in which, owing to its admittedly high practical value, diagnostic acumen is the password to the control of material.

A second divergence that it is important to recognize consists in the fact that curative medicine deals with the individual, while progressive medicine is collective in its outlook. The difference is largely controlled by the factor of time. If a man, by intent or otherwise, goes overboard, his succour comes, not from him who pauses to consider causes and effects, but from him whose hand goes forth instinctively and instantly to grasp and throw a rope. To curative medicine the patient is distinctly and very properly the very centre point of consideration, but to progressive medicine he is but an incident. To the former the patient is a human being in trouble, and the object of knowledge possessed or sought is the mitigation of this trouble, the comfort and relief of the sick man. To the latter the patient is one who exhibits phenomena of disease, phenomena to be compared and correlated with those displayed by other subjects. Samaritanism, the greatest guiding force of medicine as a whole, is in practice an ever-present and direct, and in progressive medicine a much more distant, and therefore less obtrusive, motive.*

When we consider these divergences we realize that, while curative medicine is essentially observational, it constantly repeats itself; for that reason it is safe, for both the doctor and his patient; for the same reason it tends to be in large part sterile. Progressive medicine to be fertile must from the nature of things be experimental and original. And in this connexion we must note what will be obvious to thoughtful people—namely, that experimental research upon disease may very readily come into conflict with full solicitude for the sick. When such conflict threatens, as it not infrequently does, it is unquestionably right that research should give way unhesitatingly; any other system would be intolerable. Interference giving rise to temporary discomfort, the patient willingly co-operating, may

* This and the next paragraph were criticized at the time because my meaning was not understood; the criticisms were met subsequently by amplification in paragraphs that will be found on page 35.

be permissible; interference calculated to bring risk, however slight, to a patient's health must remain unjustifiable. This element of conflict is to be noted first, because it has to be safeguarded under any system of work. No worker can reasonably expect to be relieved of full responsibility in the care of any patient remaining in his charge; but, on the other hand, it is to be realized that no investigator can be successful who allows, or is forced by circumstances to allow, solicitude for his patients to preoccupy his mind.

The divergence between the individualistic and the collective method has been considered here from one relatively simple aspect—namely, from that of the different attitudes that may be adopted towards a patient within the ward of a hospital. It is perhaps almost unnecessary to point out that each method of thought has its practical applications to health, and that these applications, the one considering the health of the individual, the other of the community, often come into direct antagonism. This long-recognized antagonism will not be discussed further than to point out that it really constitutes but one of the issues in the general problem we are considering.

The third and chief divergence between curative and progressive medicine from our present standpoint is, however, intellectual.

In his daily work a practising physician has little or no control over the questions presented to his mind; mainly diagnostic, they are brought to him for solution one after another in great profusion. His solutions are reached in by far the greater number of instances by no process of logical reasoning, but upon the basis of previous experience or upon the basis of what we call intuition, a faculty often remarkably developed. Inevitably his mental activities are chiefly of this kind. The questions are not only profuse, but diverse; the mind is jerked forcibly from one question to another. When problems relating to the mechanism of disease present themselves, as they inevitably do to the more active minds and in the earlier years, then, before a clear line of thought can be established or followed to a satisfactory conclusion, the mind is torn from its problem to be confronted with others. The practice of medicine from its very nature is destructive to consecutive thought; its continued practice weakens the very power to think consecutively, and therefore clearly, upon problems relating to the nature of disease, a matter very vital to progress. The atmosphere of the teaching

wards is tense with effort, mental and physical, but it is not an atmosphere of sustained argument. The problems faced by an investigator, properly circumstanced, are under his exclusive control; they are never numerous and rarely disconnected; his observations are made without hurry, his thoughts are equally deliberate, and so his conclusions take shape after long pondering, criticism, and revision.

The physician is forced by his daily work to decide frequently upon an insecure basis of fact and argument. In his work many of his decisions are frankly opportunist, many are wrong. But if most are right and many of the rest are harmless, then his work is a success, despite frequent mistakes. As a doctor he is sound, for his decisions, like the questions, are independent of one another. An investigator who draws false conclusions early in his research may see the whole structure of the work topple, for here conclusions follow in sequence and an early fallacy affects all; moreover, his work is placed on record and is submitted to close and critical supervision. To a physician, mistakes, if not too frequent, are of little consequence—they are indeed inevitable; they do not shake self-confidence as they do in the case of the research worker. Self-confidence is by general consent one of the essentials to the practice of medicine, for it breeds confidence, faith, and hope. Diffidence, by equally general consent, is an essential quality in investigation, for it breeds inquiry. Here, then, are chief characteristics, each necessary in its own sphere, each unsuited to the other. The two irreconcilables do not stand alone, but find natural companions. A natural companion of confidence is an easy and uncritical acceptance of statements of fact and of hypothesis; it is often coupled with a very wide and diverse acquaintanceship with other men's work and thoughts. The companion of diffidence is scepticism; it tends to be coupled with knowledge less extensive, but derived more from personal experience and analysis, knowledge more precise, and often more fundamental. The very essence of daily observation and thought in scientific work is the continual effort to discriminate as closely as possible between what is true and what is less true or actually false. The standard of truth attained by constant and deliberate cultivation among scientific men in their work greatly transcends that in any other sphere of human thought. It is the standard that allows no statement to pass without full qualification, without full display of its limitations. It is a standard essential

to progressive work, but one highly inconvenient, and even obstructive, in the practice of everyday life. The mind of the craftsman dwells on what he knows, and he delights to use and to display this knowledge. The investigator's mind dwells in the main on the unknown and puzzling, and his eagerness is often towards displaying doubts and difficulties. The adoption of a laboratory diffidence by a practitioner would soon ruin his practice. The acceptance of the easy standard of conclusion found in practice would similarly end a research worker's usefulness.

The divergences between practical and scientific medicine, and especially between their manner of thought and standard of accuracy, are in actual fact profound and far-reaching. They are perhaps as profound as, they are certainly not dissimilar to, those exhibited in the past between orthodox religion and science.

Preliminary consideration has now been given to the training available for the man who desires to devote himself to Clinical Science. It is a training developed to suit the consultant practitioner rather than the research worker. It has also been shown at length how widely the methods of work and of thought in these two spheres differ. It is the methods of curative medicine that dominate in general hospitals, and it is in this atmosphere that the bulk of investigational work upon patients is now done. Many, if not most, of the difficulties that at present face the clinical investigator arise out of the past and present control, be it direct or indirect, exercised by curative medicine over the facilities for such work and its recording. This control, absolute a few years ago, still predominant to-day, was quite natural, and indeed strikingly beneficial, in its period. If we omit consideration of quite recent times, it is true to say that investigational work in clinical medicine has been undertaken almost exclusively by, or at the exclusive encouragement of, men earning their livelihood in practice, and that practically the whole of our familiarity with the forms of disease as these are found in living human beings, and with the spontaneous manifestations of these diseases, is due to the efforts of such workers. This indispensable knowledge has been gained by the method which in the beginning of all biological science is fertile —namely, by observation of spontaneous phenomena, the method from the earliest times most natural to, and assiduously cultivated by, physicians. Eventually, however, the fertility of this method

greatly declines by a process of exhaustion, and, for those who can read the signs, this time has come in medicine.

There are, no doubt, many important forms of disease yet to recognize, important and spontaneous manifestations yet to note; but the experience of the last few decades has shown this type of work to be decreasing in fertility, while its place is being filled by other and more successful methods of exploration. It is also evident that the continued separation of disease into types can be pressed too far, and that a subdivision which may perhaps have some immediate practical use can help to conceal underlying mechanisms held in common, and thus to hinder progress in studying the nature of disease. It is abundantly clear that present progress in clinical medicine is less and less due to observing what chances to happen in beds filled by the routine demands of unselected sick people and of teaching, and more and more attributable to intensive study of selected cases in which manifestations are deliberately sought or actually provoked, or upon which new and often highly technical methods are employed, which are derived from prolonged and searching studies in laboratories. It is equally clear that successful research work in medicine is becoming more and more difficult for the man engaged in practice to prosecute; it is becoming more and more the province of men who train themselves in technical methods of work and in suitable habits of thought.

It is far from my intention to imply that work of a progressive kind cannot be carried out exceptionally by men engaged in practice; on the contrary, there are some directions—for instance, prognosis—in which investigations can rarely be pursued with equal prospects of success by any other group of workers. There *is* the intention to imply that in general neither the training nor the daily routine of practice conduces to the type of investigational work which progress in medicine now chiefly demands; and that the practice of general medicine by continually acceding to opportunist and hurried conclusions definitely tends to unfit a man for research. Conversely, the prosecution of research in medicine tends in general to unfit for practice; and the more whole-heartedly and continuously research is undertaken, and the deeper the research goes, the less fitted the worker becomes for the care of unselected patients. This is clearly recognized by workers themselves, and, as long as there is no reasonable prospect of careers in research, will continue to discourage them from developing full research efficiency. It

is in part for this reason that research in medicine is most compatible with specialism; the opportunity to undertake progressive work is in fact often the main justification for specialism. It is not possible in research to maintain full familiarity with general medicine, with its ever-changing devices and palliatives, when so much of both time and energy are concerned in studying matters that have no immediate relevancy to practice, and in laboriously and accurately collecting data and critically forming conclusions. The idea, more prevalent perhaps in certain foreign countries than in our own, that research is a suitable introduction to general consulting practice, is not only intrinsically unsound, but has proved itself mischievous. After his qualification a man, by attempting investigational work for a short space, and thus familiarizing himself with methods and especially with the difficulties of gaining new knowledge, will often broaden his views and increase his capacity as a teacher. Ability to undertake research, however, apart from its providing a possible gauge of intelligence, is no criterion of practical capacity; and experience in research, especially on the laboratory side, has little or no value in practice; consequently, when preliminary years are spent in large part in this way, they displace years of more valuable practical experience. The proper sequel to such research is not practice but further research—in short, a career largely concerned with investigation.

The serious aspect of the widespread encouragement of transient research is that, while such work rarely possesses scientific value, yet the reporting of it has acquired a recognized commercial value, and its realization in the latter way becomes much more often than not the real goal in view. The goal referred to is in certain circumstances a perfectly legitimate one. It is a most desirable and healthy thing that good teachers should use the medical press to inform; it is right that contacts between consultants and general practitioners should become established by this means, and that through periodicals new work of established value should be brought to the knowledge of the profession as a whole. But the present system is obviously capable of, and is actually subject to, grave and frequent abuse; the abuse consists in the redundant publication of articles intended to pass muster as records of serious original research. Now the editors of the journals concerned are for the most part either whole-time editors or are actually engaged in practice. In neither case are they trained by long practical experience of

research to deal with such articles critically or to judge them well; their daily contacts are with curative rather than with scientific medicine. Thus it comes about that there is little or no attempt on the part of editors collectively to stem the tide of pseudo-scientific publication; its amount is controlled almost exclusively by the desires of writers, and the tables of our libraries groan under an ever-increasing weight of periodicals. Research is widely exploited for professional ends; records of imperfect and actually unsound work find an almost open path to the printing press; reports of purely repetitive work have reached an extraordinary and most undesirable magnitude; many papers are republished almost in duplicate. The effect of such publication has passed beyond the bounds of menace; it has grown to form a distinct barrier to progress, with which more serious workers recognize they have to contend. The baneful effects are twofold. By its mass it conceals work that has value; by its quality it undermines the general standard of accuracy in observation and thought. Current teaching in the wards and textbooks, current methods of thought in practical daily work, are built in very large part upon the basis of published articles, past and present. The quality of such articles is a matter of vital concern to professional work, both directly and also indirectly, through its influence on progress.

Main functions of workers in progressive medicine are to consolidate old and to explore new ground, to watch the general tendencies of teaching, and to bring the results of their work within the reach of practitioners and thus to practical application. From several of these standpoints there is, in considering the present position of medical research, something more to be said.

In research, one step forward leads to another; researches begin from an accepted foundation and proceed. It behoves the worker to examine closely the foundation from which he starts and to make sure that this ground is solid. In research that directly concerns patients this preliminary work of consolidation is particularly arduous. A physiological worker can in very large measure start from the records left by previous workers; in medicine, as things now stand, this procedure is less safe. The relative unreliability of clinical statement of fact, and especially of conclusion, comes from several sources. Clinicians who record facts and views are not chosen, as for the most part are physiologists, for their ability to use precise

methods of observation and of thought; in medicine inaccuracies in statement of fact are, owing to the material used, more difficult to detect, penalties for mistakes are less severe; the standard of truthfulness, in the sense previously indicated, is far less rigid, than is the case in physiology. A very grave defect in medical publication, from the standpoint of progress, is the dearth of systematic statements of evidence. In a very large proportion of instances, satisfactory and recorded evidence for current clinical beliefs is either most difficult to discover or is non-existent. Far too much stress is laid upon suggestion; far too little upon the proof. In this relation it is sufficient to mention therapeutics, but the defect is not confined to this branch.

There is a great work to do in close and critical revision of present beliefs in every branch of clinical medicine. Upon the extent and thoroughness of such consolidation the future rate of progress will in large part depend, as it has depended in no small measure in the case of cardio-vascular knowledge in the past few decades.

It is essential to concerted progress that there should be more exact definition of terms applied to disease and its manifestations, for close definition would bring increased clarity and would diminish the prevailing confusion of tongues. It is essential to progress that closer attention and study should be given to the precise laws relating to cause and effect; that they are widely misunderstood is illustrated sufficiently by a common tendency to enumerate under " etiological " headlines many and diverse causes of a single effect. It is essential to progress that observational work should be far more widely supplemented by the experimental method and by its manner of thought, and particularly that a full understanding and rigid use of controls and comparisons should develop extensively. Finally, it is essential to draw more closely together the bonds between physiology and pathology on the one hand, and clinical medicine on the other.

These activities, these reforms, have become beyond the power of curative medicine; they can be properly encompassed only by men who are not beset by numerous and diverse daily tasks, who have ample leisure for study and for reflection, who in training have acquired special and suitable experiences and have deliberately cultivated those habits of mind that conduce to successful investigation, who, in brief, make research a life work.

Remedies

The present weaknesses of medicine, regarded from the standpoint of its progress, have been discussed from those points of view that suggest remedies.

The great contribution of curative medicine to knowledge of disease as displayed by mankind has been emphasized. Curative medicine has done more than that. The allied sciences physiology and pathology are its direct offspring, and have ultimately drawn their chief inspiration from it. Not many decades have passed since it became recognized that physiology must assume its own control; within a brief period its progress as a science became remarkable. Pathology has become more recently, though less completely, free, and is making not dissimilar history. These associated sciences are rapidly outstripping clinical medicine in new developments. The fault must be assigned to medicine, which, failing to pull its weight and keep abreast of the rest, threatens to disrupt the team. It is for clinical medicine, the most dependent but the most important of the group, to maintain contact and to draw renewed strength from its more vigorous progeny, and not, as now, to lag preoccupied. The remedy lies in the emancipation of Clinical Science from the controlling influences and demands of routine practice; for therein lies its chief weakness. Just as it became necessary to the full development of physiology, so it has become necessary to that of progress in Clinical Science, that its workers should enjoy independence.

The first step to be taken is the training of a group of workers with a view to their devoting their lives primarily to research, making disease as this occurs in man the centre point of their studies. The essence of the worker's training in all the concrete sciences consists in the development of correct methods of work and thought; accuracy in observation and in measurement; in biological science especially, a full understanding of the experimental method; a firm grasp of the nature and use of evidence; to know how to test and how to weigh, to know how to sift out error, to be able to judge the truth or falsity of statements. From the time of qualification the worker we are considering should enjoy full freedom to develop along these broad and recognized lines; he should have the opportunity to acquire such technical methods as may be needed in the pursuit of given problems, and freedom from the recurring fear of failure as an

expert and general diagnostician. He should have ample leisure to maintain close contacts with physiology; for, if he becomes a successful worker, he will not only put physiology to good use, but, like his predecessors from Harvey's time down, he will help the growth of physiology, and will stimulate its growth in directions in which from time to time it will require stimulation—namely, into the more biological channels.

A simultaneous step must be the assurance that careers will be provided for those who train themselves successfully. And careers must be assured to individual workers at the earliest possible moment. Young and competent men cannot be expected to hazard the best years of their lives in long and special training without reasonable prospect of corresponding careers before them.

There are at present a very few academic posts, the units, concerned primarily with organized teaching in the schools, appointments of uncertain development and destiny, but offering, it is true, limited possibilities of research. Sure it is that as long as these posts are concerned with the function, important as it is, of teaching general medicine, their capacity to advance medicine by research will remain limited. There are also one or two purely research posts with full facilities; but these are personal appointments, and, being such, cannot be regarded as possessing stability.*

It is unreasonable to expect, it would be unwise to advocate, the sudden creation of many posts; for there are not the adequately trained men to fill them. But the vicious circle now existing, whereby there can be no posts till there are trained men, and no trained men till they can see the prospect of posts, must be broken. It can be broken by clear statements of policy that posts will be made for those who render themselves competent for such careers. The posts must be full-time posts, for full-time work can alone ensure the leisure for efficient study, and freedom from that most distracting of thoughts, the imminence of an alternative career. The workers must be secured free and full access to the material they desire to study, and full medical charge of that material; work upon patients in another's charge fails to give sufficient freedom of control and dulls the fine edge of personal responsibility. General hospitals provide the necessary wealth and diversity of material; a teaching element in general hospitals sorts the material and renders it

---

* A number of posts has been established on a permanent basis since this article was written.

quickly available. The presence of research departments in general teaching hospitals stimulates teaching, and links science and art. For these and other reasons these hospitals are very probably the most suited to house such departments; but if that is the decision these departments should enjoy independence of the more purely curative departments, they must in no sense be subordinated to these, as is the strong and natural present tendency. The need is for *research physicians*, and their departments should include adequate but not abundant beds, and proper laboratory facilities; there should be arrangements for exchange of patients with other departments of the same institution, ensuring by this means supply of material and relief from the care of patients no longer suited to research. Their teaching should be limited to the demonstration of new methods and observations, and to the exposition of new ideas.

The future creation of research posts requires the present promise of financial backing; their creation in teaching institutes would require the early promise of active co-operation by these institutes; the alternative is the foundation of special hospitals or institutes.

The future of medicine lies with the younger and recently qualified generation and with their successors. It is within their power to bring Clinical Science in this country to a great revival. It is for them to bind themselves into a new association, which by influence and by example may carry the standard of observation and thought to precisely the same level as prevails in allied sciences, and firmly to link up with these on the one hand and with practical medicine on the other. To this end existing associations of physiologists and pathologists will not suffice them. There are problems arising out of contact with patients that are of little or no interest to the associated sciences. It is necessary that Clinical Science should enjoy its own intimate and untrammelled discussions. It is necessary that it should constitute its own courts of inquiry, of criticism, and of approbation. It is necessary to its full development that it should have its separate scientific societies, should control its own publications, and should be able to confine its original reports to journals restricting themselves exclusively to original matter of proper quality.* It should develop its own training ground for new recruits. It should achieve independence and freedom.

---

* The Medical Research Society, founded in 1930, is now achieving this purpose. The same society at my desire assumed editorial control of the journal *Clinical Science* at the end of 1938.

THE RELATION OF CLINICAL MEDICINE TO PHYSIOLOGY FROM THE STANDPOINT OF RESEARCH*

The relation between physiology and clinical medicine might be treated from a number of distinct and interesting points of view, but my remarks will be confined to their relation from certain standpoints of research work. This subject has long interested me deeply owing to the intimate contacts I have enjoyed for many years with both these sciences. The occasion is one which obviously affords an opening for compliments to physiology. They would be compliments easy to make and it would give me great pleasure and satisfaction to utter them; for my admiration for the advances physiology has made in recent times and for the great and beneficial influence it has exerted upon medicine is unbounded. Though it would give me personal gratification I do not see that it would fulfil a very useful purpose, for both physiologists and clinicians are already well aware of the services that physiology has rendered and continues to render to medicine. I feel sure, if what I say to-day includes little direct praise of physiology, if for once it gives clinical medicine the prominent place, that my physiological friends will recognize that my sympathy and esteem for physiology remain unabated. In speaking of the relation chiefly from the standpoint of medicine, what is said is dictated by the developments of recent times, which call me to pursue an idea that expresses a great need of the present day. It is that clinical medicine should make a determined effort to take its proper place as a progressive science on an equal footing with physiology. And I look confidently to physiology for the support which I know it will generously give, on every occasion when it can do so, towards the accomplishment of this purpose.

---

* Opening a discussion in the Section of Physiology at the Centenary Meeting of the British Medical Association, London, 1932, and first published in the *British Medical Journal*, December 10th, 1932. Its purpose was to bring the views expressed before the leading physiologists of this country, and to obtain their sympathetic consideration of the new movement.

Mutual Inspiration

Although this aspect will not be dealt with at any length I cannot refrain from placing first the statement that physiological work forms and must continue to form a great stimulus to clinical medicine. The fact is universally acknowledged and I pay my own homage unreservedly to it. When it is said, as it often has been said, that modern medicine is being built upon a foundation of physiological knowledge, an undeniable truth is stated. Abnormal phenomena that we observe in diseased subjects must be interpreted usually as interferences with normal function, and therefore in terms of normal function. But, as there will be further occasion to notice, this is only a fraction of the truth. While clinical medicine draws inspiration from physiology, so likewise physiology draws inspiration from clinical medicine. Physiology has benefited greatly in the past, and continues to benefit still in two ways: first because a very large proportion of physiological work is directly or indirectly suggested by clinical problems; and secondly because clinical workers, in their search for explanations of the abnormal phenomena, which they witness, often themselves become heavy contributors to physiological knowledge.

The most obvious illustration, and one which will serve the double purpose, is that of Harvey and his work. Harvey has been called the "Father of Physiology," but Harvey was a physician. The fact that there were no pure physiologists in those days, and that Harvey might be regarded as representing physiology in his time, is irrelevant to the point that is being made. I am using this great physician's work as an illustration of how contact with patients inspires and leads to research upon the normal functions of the body; it is a perfect illustration of how clinical medicine can react upon related science: in this instance actually beginning to form a science. Whatever else Harvey did he undoubtedly discovered the systemic circulation; and it is very apt here to point out that this discovery in particular, and the more complete statement of the circulation into which he wove it, was based in very large part upon experiments carried out upon the human being, the first object of research to which a physician naturally turns.

To illustrate from modern times presents no difficulty.

The origins of our knowledge of the chief ductless glands and

of growth factors in food were clinical. The work of Henry Head upon normal sensation was prompted throughout by clinical observation. Finally, to cite an example of less direct contact, the brilliant physiological researches of our president (Sir Henry Dale), which have led up to the discovery of histamine and acetylcholine as normal and probably most important products of tissue activity, began in a search for the active principles determining the therapeutic action of the remedy ergot.

At the moment two chief objects are in view; in the first place to emphasize the importance of research upon phenomena witnessed at the bedside, so that young men of ability may be helped to realize, or supported in their belief, that the clinical field of work is a most fruitful field, not subservient to but often guiding physiology; and to show that it is most important to the welfare of physiology that clinical medicine should be strong.

Three Important Branches of Medical Research

A statement that medicine is founded upon physiology is, as has been said, but a fraction of the truth. There are vital branches of clinical knowledge to which physiology contributes little if at all. There are three chief ways in which clinical progress is achieved, and these may be reviewed briefly. They are: (*a*) the discovery of disease, that is the identification of disease and its natural history; (*b*) experimental work on clinical cases; and (*c*) the application of physiological ideas and discoveries. The third of these is in fact the only one that is fully relevant to our discussion to-day; but it is impossible to obtain a clear view of the relations of clinical medicine and physiology without understanding all.

(*a*) *Discovery of Disease*

This term is used more because it is convenient to my purpose than because it is accurate. It is a phrase standing for the clear description of specific diseases or states, so that these may be identified unmistakably for what they are by others. The method here used is chiefly though it is not exclusively observational, and it is comparable to the isolation of specific species in biology. Studies relating to the cause and meaning of a

disease or state, studies relating to its treatment, can scarcely begin until the first step is taken, until the identification of the disease or state has been accomplished. It is a vital step, but its importance is frequently overlooked by those unfamiliar with clinical methods. This matter may be brought home perhaps most readily by reviewing the history of cardiac diseases during the last thirty or forty years. The practical management of cardiac diseases has changed and has improved remarkably during this period of time. What then have been the chief discoveries leading to these changes, which have been as great and beneficial as those occurring contemporaneously in any branch of medicine or surgery? Many still harbour the notion that a chief advance has been the analysis of cardiac mechanism, beginning, if this is fancied as the proper starting-point, in Gaskell's work on the frog and tortoise heart. It is impossible to acquiesce in this idea. In so far as the mechanism of the heart is concerned, the chief advance has been the successful subdivision of different irregularities as these are exhibited by human patients, coupled with the subsequent study of the natural history of these irregularities and of the action of remedies upon them, which was rendered possible by this subdivision. This work was independent of the final analysis of disordered mechanism, in the sense that it did not require that analysis; in actual fact it outstripped the analysis of disordered action in almost all important instances; it was carried to completion in many, could have been carried to completion in all, instances without the analysis. Thus, the chief practical discovery of our times, the almost specific action of digitalis in cases of gross irregularity of the heart, was accomplished by James Mackenzie's splendid work long before we knew this irregularity to be due to auricular fibrillation. And the discovery that it is due to auricular fibrillation has been responsible for little or no change in the treatment of the disease either by digitalis or by quinidine. The isolation of the irregularity due to auricular fibrillation from other irregularities was purely a clinical discovery and unprompted by physiology; it could not have been anything but a clinical discovery. The isolation of this and other irregularities, with the natural sequence—namely, the rational use of digitalis and the allied drugs—has been perhaps the most revolutionary change in the management of heart cases in recent times; and the benefits that have been derived from it are now incalculable; they unquestionably exceed the benefits that

have been derived from the discovery of insulin, owing to the far greater prevalence of auricular fibrillation as compared with diabetes.

It is possible that the discovery named should not occupy the first place; it may be that other discoveries now to be named will prove of equal or greater consequence; it matters little from our present point of view, for these are also clinical and not physiological discoveries, and could not be otherwise. I name the life-history of that protean disease rheumatic fever, as it is now known, and the great additions that have been made to our knowledge of the times and ways in which this disease invades the heart. It is to be remembered that rheumatic fever is the chief cause of heart disease. I name the identification of subacute bacterial endocarditis, of coronary thrombosis, and if the years may be a little extended backwards, the recognition of hypertension in man and its consequences, as also amongst the chief discoveries of modern times leading to changes in our care of cardiac patients. Most if not all of these clinical discoveries would have been made in almost exactly the same form had there been no contemporary physiology. In discussions relating to the characters of discoveries the word " fundamental " is often used. Using the word in its accustomed sense, we may say that the discoveries I have named are fundamental to Clinical Science.

(*b*) *Experimental Work on Clinical Cases*

In an article written a few years ago* I had occasion to indicate that progressive medicine can cling too exclusively to be fertile to the observational method, by which is meant the simple witnessing and recording of spontaneous phenomena; but that it must adopt more widely and use more carefully the experimental method. It is self-evident that the observational method, most fruitful as it has been even in recent times, must through mere exhaustion become less prolific, whereas the experimental method whenever it is applied to biological science is found to open up new channels of progress. I have since seen no reason to change this view, though my remarks have been read by some with insufficient closeness and in consequence have sometimes been misinterpreted.

---

* The article precedes this one.

The misinterpretation arises out of a wrong understanding of the word " experimental," which has come to be associated too exclusively with animal experimentation. The two methods, observational and experimental, are of course closely interwoven and a useful purpose is not always served in attempting to separate them. In experimenting we make observations, not upon events that are happening quite spontaneously (observational method), but upon events that are provoked or influenced by the interference of the experimenter. The method is by no means new to medicine. Many physical signs that are used clinically are won not by purely observational methods but by experiment. If a muscle is stimulated by an electrical shock and it is noticed to respond, that is an experiment. If a tendon is struck sharply and the knee-jerk results, that is also an experiment, and one that can be undertaken under a variety of conditions controlled by the observer. Tests that are employed to identify and isolate diseases are becoming more experimental as time passes. Therapeutics has always been and always will be almost purely experimental in its method; but it has not always adopted the full precautions of what has come widely to be called the " experimental method." My plea is that these experiments, inevitable as they are, should be conducted in such fashion that the result does not remain in doubt, they should be controlled as experiments are controlled in a physiological laboratory. Such controlled experiments will mean to the individuals, upon whom they are undertaken, a little sacrifice of time and perhaps of comfort (it goes without saying that they must entail no risk) in order that the observations on the few may benefit the many. That there may be no possibility of further misunderstanding of this important matter one or two of the precise examples that I had originally in mind may be cited. About twenty years ago a patient was admitted to hospital suffering from rapid ventricular action due to a fibrillating auricle. He was put to bed but was given no digitalis until ten days had elapsed. This was done to ascertain if, and by how much, simple bed treatment would lower ventricular rate; and to avoid the pitfall of attributing to digitalis an effect for which it was not responsible. It was desired to ascertain precisely what the effect of digitalis was in such a case, and the withholding of digitalis for a preliminary period was necessary to arrive at the true answer. A physician so acting, though acting with the highest motives, could not be said to be " pre-

occupied " by solicitude for his patient.* Regarded purely from the standpoint of an isolated patient, the withholding of that drug, even for a few days, is not easily defensible; from the standpoint of inquiry into the best means of administering digitalis to many future patients no other plan would seem to be defensible.

If I have under my care a patient with Heberden's angina of effort, and I ask that patient to do in my sight what he is in the habit of doing daily—namely, to walk a given distance —and I do it so that I may ascertain precisely how far and how fast he must go before he begins to feel pain; if I now induce him to repeat this test under different conditions of my choosing, as before and after administering to him nitroglycerin, then I am clearly making full use of the experimental method. And in this manner information is gained about the action of the drug, which is valuable not only in the general sense but to the particular patient upon whom the tests are made. Such experimental work, in the hands of those fully realizing their responsibilities for the welfare and well-being of their patients, is playing an increasingly useful part in progressive medicine. It is a method which, properly employed, is full of promise. It is sometimes referred to as the " physiological method," but that is a loose phrase, and dependent simply upon the accident that physiology makes of it such abundant use. There are certain methods of work that are suitable and common to all biological sciences, and whether work is carried out in ward or in laboratory. The experimental method belongs no more to one field of work than to another; and in parenthesis it may be said that the use of the terms " ward methods " and " laboratory methods," in so far as it implies that the methods are essentially distinct, is to be deprecated. There is but one method and that is the true method, and it is applicable equally in ward or laboratory. But my main purpose here is to indicate that clinical medicine has its own proper field of experimental work and that in this field it is not borrowing from or leaning upon physiology. It is work essential to the progress of practical medicine. It is work that is carried out upon patients suffering from disease; it can be undertaken only by clinicians, and these should be clinicians having special facilities and training, and

---

* The reference is to a phrase used in an earlier article (p. 18), criticism of which is here met by amplification.

having judgement to know how far experiment is consistent with the complete welfare of the patient.

(*c*) *Application of Physiological Ideas*

The great contribution of physiology to clinical medicine is the help it gives us in understanding the mechanism of disease, allowing us, as previously indicated, to interpret disease in terms of altered function. Thereby our conceptions of disease are brought to greater exactitude and we are enabled to regard the phenomena displayed by our patients more rationally; the gain to accuracy of thought ultimately influences medicine profoundly. From time to time, too, a laboratory discovery, by supplying a necessary link, leads almost at once to a therapeutic discovery manifestly of first-class importance. The discovery that insulin enables the tissues to utilize sugar, and that a substance in the liver may regulate the formation of red blood cells, will at once come to mind. What is now added is in no way intended to disparage the importance of these laboratory discoveries. They are of the highest importance. But if the matter is left at that stage the relation of physiology to medicine, as illustrated by such instances, will be left only in part explored. The use of insulin for diabetes, the use of liver extract for pernicious anaemia, has not depended purely upon laboratory experiment. A first and vital step in each case was the discovery, the isolation and clear description of the disease concerned—and, incidentally, in each case this knowledge of the disease reacted favourably upon correlated physiology. A last and vital step in each case consisted in the testing of what might perhaps prove to be a remedy upon selected patients. Thus, the therapeutic end-result was accomplished by a chain of work and that chain was forged of links, some clinical and some physiological, but each and all indispensable to the integrity of the chain. In such cases the emphasis often comes to be laid almost wholly upon one of the links and this a physiological link. Perhaps this is natural, but it is not always quite reasonable. That it is not always reasonable is shown by the fact that the high praise for the physiological link does not always come immediately but awaits the clinical link or application; evidently in such instances the praise is not based upon the intrinsic merit of the physiological discovery but largely upon its end-result. In appraising the merit of laboratory work too

much stress is often laid upon the end-results to which the work leads; actually the application of his work is not often foreseen by the worker himself. It is not his deliberate design, except in the sense that he may safely assume that all knowledge will at some time become applicable; but if that is an inevitable circumstance, it is one for which the worker is not responsible. When a physiological discovery is applied to medicine, the application is in fact clinical, and it is not infrequently beset with difficulties equal to or even greater than those of the original physiological discovery; to clinical science therefore the credit of the application should go in full. If, as sometimes happens, the physiological element of the discovery is selected for undue praise, then the importance of the clinical contribution tends to be overlooked, to the discouragement not only of the clinical workers concerned, but of clinical as compared with physiological investigation.

Let us, however, put this question of the apportionment of credit on one side, and look at the matter more broadly and without placing physiology and clinical medicine in rivalry. The whole position is summed up, so it seems to me, in a few sentences. Knowledge that is to be applied usefully to the health of mankind will almost always come by a series of steps, the first of which is the recognition of the human need, the last of which is the application of a test directly to the human problem. It is in the nature of things, however many steps may intervene, that the first and last must be clinical; as it is also in the nature of things that almost all important physiological discoveries that are immediately applicable to the treatment of disease have their original source in clinical observations.

Concluding Remarks

It has here been indicated that there is vitally important work, appertaining to the study of human disease, that can be undertaken only upon patients and by clinicians. It seems unwise unduly to prolong the emphasis that it has become the habit to lay upon laboratory contributions, and especially physiological contributions, to medical science. When physiology was struggling for independence and for position, this emphasis served a useful purpose and left Clinical Science unembarrassed. But physiology is now in a much stronger academic position than is Clinical Science and is overshadowing

it. The stress laid upon physiological discovery is due in part to the fact that the full importance of Clinical Science is not recognized by those who are inclined to see in physiology more of the basis of practical medicine than physiology actually provides. It is wrong, and it is a misfortune, that young men whose life-work lies in clinical medicine, should be brought to feel that clinical is less fruitful than physiological research; or that they should be forced to think that they can win their spurs as research workers only in the laboratories and not by work on patients. An over-emphasis of one branch of physiological work plays a particular part here. This most fruitful branch is over-emphasized because it has become necessary to defend it in the justification of animal experimentation; but the position it so often takes in the public limelight is inimical to the best interests of medicine as a whole.

It is most desirable, if they are mutually to inspire, encourage and help each other, that Clinical Science and physiology should pull abreast in the team of medical sciences; it is clinical and not physiological science which now needs the stimulus to a new and more vigorous alignment. Clinical Science must possess itself of a phalanx of research workers of ability and full training, who will hold the fort for this branch of medicine, build up again its scientific prestige, and maintain its privileges in a manner that those engaged in the busy routine of daily practice cannot hope to accomplish. There are benefactions and bequests to medicine that are sometimes made in such terms that they are undoubtedly intended for clinical research, for the " discovery of disease "—a first and essential step, as we have seen, towards the study of its cause and towards the alleviation and cure of disease. Such benefactions tend to become deflected to physiology and this tendency is probably inevitable—I am not sure that it is not desirable—until Clinical Science can enforce its claims by sheer merit. The successful enforcement of the legitimate claims appears to depend chiefly upon the establishment of suitable posts in clinical research and the formation of a group of full-time workers, who can hold their own in method of work and of scientific thought with workers in any other branch of medical science, and who are prepared to devote their lives to the study of disease without forethought of practice, and to the consolidation of that branch of science which is their own. The kind of training that is desirable for men aspiring to such full-time posts, the facilities required for

their work, I have already attempted to outline in an earlier address.*

Finally let it be clearly stated that what is in mind is not a separatist movement from practical medicine, but a linkage between this and physiology and pathology, through a body of workers who have the necessary training and sufficient leisure to understand and apply the knowledge these sciences are gaining, and to interpret them to those whose energies are devoted to the arduous task of recognizing and alleviating the troubles of sick people. The linkage, as I indicated many years ago† and as I still visualize it, is largely through human physiology.

---

* See p. 1.

† See p. 13.

THE HARVEIAN ORATION ON "CLINICAL SCIENCE"*

ALL in this College know that William Harvey discovered the circulation of the blood. But the movement of blood through the lungs had already been suggested by his predecessors. Harvey discovered the systemic circulation, and realized that this and the pulmonary circulation are but integral parts of one continuous movement. When it is asked what we owe to Harvey this forms the almost constant answer. Yet I venture to state that it is not the achievement which should chiefly be acclaimed, or that it should not be acclaimed in this form. There is little doubt that Harvey's discovery could not long have been delayed. Other workers had come very near to suspecting the fact; with the microscope once introduced it could not remain concealed. How and when the first clear statement would have come, had Harvey remained silent, would be a matter of profitless conjecture. It actually came from Harvey, and we are content to rest on that fact. Revolutionary as was his discovery in its time, far-reaching in its subsequent developments, yet the conclusion was not the best that Harvey gave us. The outstanding quality of Harvey's work lies in his statement of a case, in the lucid nature of his argument, and the finality of his proof. He presents to us a train of methodical and exact observation and experiment, carried forward hand in hand with close and finished reasoning, brilliant for its simplicity and for its power of conviction. His book is a romantic example of scientific exposition, unsurpassed at his time and since his time. *De Motu Cordis* holds its reader entranced, while each stretch of the path brings a surer promise of the approaching vista; and when the path ends it ends on the hill-top, and there is unfolded to our gaze a land that is ours with certainty, ours for all time. This culmination brings with it a sense of security and accomplishment that fills us with profound and lasting emotion; and we go forward to hold what he has given us with a great thankfulness to this pioneer, who has

---

* Delivered before the Royal College of Physicians of London on October 18th, 1933, and first published in the *British Medical Journal*, October 21st, 1933.

not only led us to a new country, but has inspired us and taught us how farther to explore.

Harvey has been termed the Father of Physiology; he was much more than that, for his child was Clinical Science, out of which physiology and pathology were afterwards born. Prompted and inspired by clinical observation and by dissections of the dead body, he proceeded farther by the method of experiment, using both man and animals, and harvesting and binding into one whole his abundant evidences. He bequeathed his method to us in his immortal book.

Since his time knowledge has grown apace, and where there was one science there have sprung up many. Clinicians in the age following Harvey became largely absorbed in the description of disease; in the hands of men like Sydenham this fascinating branch of knowledge, derived from observation, rapidly expanded. Though prosecuted sporadically in this country among physicians, Harvey's method fell amongst them into relative disuse; until during the last century, being relegated, it was seized upon enthusiastically by a small band of devotees and shortly became the basis of the present science of physiology. The budding off of physiology is almost within memory. The process of disruption has continued. First physiology abandoned medicine, and in going took largely from medicine its function of studying the normal processes of the body; it also took something more important, for clinical medicine seemed to relax its claim to the use of the experimental method, which it had forged and which physiology, its offspring, now chose as its own chief weapon. Pathology has budded or is budding off, assuming for its part the study of the mechanism of disease, human or otherwise, and in almost all its aspects. This disruption of medicine, as it was originally constituted and as Harvey knew it, may be the inevitable outcome of its growth, may profit physiology, may profit general pathology, may even profit the medical sciences when these are viewed as one whole; but it is a process which can be carried too far, and it then becomes, is becoming, detrimental to the development of the clinical branches of work.

It is necessary for medicine, still the parent and still largely possessed of guiding authority, to call a check to this subdivision of her estate. It is necessary that steps should be retraced and that territory originally possessed should firmly be reclaimed, so that medicine should not be narrowly confined; it is requisite that medicine should renew its strength to wield its chief weapon,

the experimental method. It is to the accomplishment of these ends that the establishment of Clinical Science in its full scope is to be regarded as so important. But if this science is to become established it must not be formed into a third unit, which shall presently detach itself and move off to a position of isolation; it must remain firmly within and develop as an integral part of the whole organization. And to secure this most desirable and permanent unity it must establish itself, not in laboratories of our medical schools, but actually within our hospitals, centring upon the wards and out-patient department, and using laboratories merely as adjuncts and storehouses of necessary equipment. Above all, we must guard ourselves against the obsession, tending to grow up in recent years, that useful discoveries are purely the prerogative of laboratories.

The General Scope of Clinical Science

The proper practice of medicine has been built up, and continues to be built up, on a complex basis. The general mass of our knowledge rests upon a tripod, the three limbs of which are each essential to the stability of the superstructure; these limbs are:—studies of living men in health and disease; studies of dead men; and correlated studies undertaken upon the lower animals.

The full scope of Clinical Science, as the work of Harvey first defined it, embraces all these three. The central and unique studies of Clinical Science, most fundamental of all studies pertaining to the practice of medicine, are those which deal with living men. But these cannot be divorced either from anatomy or from experimental work on animals without embarrassing all three, and without seriously weakening the whole edifice.

It is quite necessary to the proper progress of the clinical branches of study that they should be most strongly linked with physiology, with morbid anatomy, and with what has been called experimental pathology. The linkage can be an enduring one only if Clinical Science continues to command opportunities of studying both dead bodies and living animals.

It is essential that those who have held charge of patients and have studied phenomena in the living should themselves, and not through skilled deputies, explore the tissue changes which may underlie disturbed function; for while the skilled deputy may more accurately describe and name those changes

in the tissues upon which he chances, he cannot enjoy either full opportunity or the full inspiration to correlate function and structure. It is by these correlations that the meanings of many manifestations during life are explained; it is by correlation rather than by simple study of the cadaver that the meaning of illness and the cause of death are often or usually to be explained.

It is essential that those who in studying human patients perceive opportunities of putting questions to the test of animal experimentation should themselves engage in such work; that correlation should not be left to the chance meeting and union of clinical and laboratory studies; that the spirit which moved the original inquiry should live, vitalizing and directing the whole work in its progress along a broad path towards a practical goal. To divide or attempt to divide medical research into ward research and laboratory research is narrow and mischievous; it is a profound error to believe that there is any essential difference in general method, however different may be the technique.

The physicians and surgeons of our great teaching hospitals become more and more dependent upon technical experts. How easy, as the problems suggest themselves at the bedside, to accept the view that it is fitting that these highly trained and fully equipped men should undertake the investigation. But the result, so often, is that from the first the research is forced into a narrow channel of technicality from which it fails to emerge, and the work ends in one of those reports which, scientific though they may be in their cold logic, unfold no story, and remain uninspiring and lifeless. The fascination and importance of the problem in its wider and practical implications have been concealed, the inspiration that should drive to its solution has been lost, because the problem is no longer seen in its full perspective, a problem primarily concerning the living, in part concerning the dead, and in part deriving from the laboratory. It is just this integration that is of so much consequence to the vitality of medical research; and this integration is, and always must remain, chiefly within the special province of Clinical Science. As there has been occasion to state previously,* knowledge that is applied usefully to the health of mankind will almost always come by a series of steps, the first of which is the recognition of the human need, the last of which is the application of a test directly to the human problem. It is therefore in the

---

* See p. 37.

nature of things, however many steps may intervene, that the first and last shall be clinical. He who can see the source of the problem, who can appreciate the fittingness of its final solution, is uniquely fitted to guide the whole train of thought and of inquiry.

Harvey investigated the human body in health and disease, living and dead, and used animal experimentation to supplement his other studies. This is our birthright, derived from him, and we must not depute our proper tasks. A great need of the medical sciences in the present stage of their development and interrelation is a group of men, primarily clinicians, but fully accustomed by training and by daily experience to wrestle with scientific problems; men who, in place of relatively complete and accurate knowledge of some purely laboratory science, hold as the first part of their equipment intimate acquaintance with the relevant diseases as these are seen in living men, who have also acquired sufficient knowledge of related physiology and of general pathology, who have the aptitude to acquire quickly and well the necessary technical knowledge and skill, thus to enable them to grapple with the problem both in its detail and in its wide aspects, and to drive successfully to a practical goal.

But such work, it will be clear, requires not only ability, but it requires a man's full energy; it is not the type of work to which those busily engaged in practice can give themselves in leisure hours with full prospect of success. Physicians do not now live as Harvey did in times of easy clinical activity, but in days of much heavier and more complex routine. So if the purpose in mind is to be brought to accomplishment it will be necessary to set free men having aptitude for the work outlined, to form a phalanx of trained clinicians who shall bring Clinical Science to a new pitch of scientific efficiency and hold it there.

From this statement of the general scope of Clinical Science and its aspirations we pass on to survey, not exhaustively, but selectively, certain studies upon which Clinical Science may profitably engage.

Directions of Investigation

Studies of the Disease

Description and Definition of Disease.—Coming down to us in part from remote times, chiefly from the great clinicians of the seventeenth, and especially the eighteenth and nineteenth,

centuries, is this tradition, the discovery of new disease, consisting of the separate description and naming of recognizable diseases. This recognition of separate diseases originally based itself upon close resemblance between manifestations of ill-health in separate people, with or without the additional evidence of a common morbid lesion. At first such discovery was a matter of relatively simple observation, as was the identification of biological species; but it would very naturally become less simple as those of most constant form were eliminated, and the number of those to be recognized consequently diminished. While requiring freshness of sight and mind, so that the opportunity might be seized when it occurred, nevertheless, progressive work of this kind has been and must continue to be a matter largely of good fortune, as when several patients of one type present themselves for examination in quick succession. To-day work along these lines could not be a planned investigation; it has become incidental to a long routine directed to other purposes. There may be many forms of disease yet to describe, but it is manifest that most diseases of simple types have been named; there is evidently work still to do in recognizing the less obvious types. Moreover, there is very important new work to do, if one may express the matter a little differently but more clearly, in *defining* conditions of disease. This defining, which is no more than accurate naming, is quite fundamental to many studies that must be undertaken in the future. The very attempt to define often fires a dormant spark of inquiry. This method of working towards precise definitions, and ultimately from them, is both instructive and prolific; it leads to the belief that diseases are named much too often upon a simple, and perhaps loose, grouping of symptoms, and that closer analysis of mechanism will lead in the future to much revision of terms. Definitions cannot be made satisfactorily in terms of grouped symptoms; definition brings us quickly to intimate questions of causation. When it comes ultimately, as it comes inevitably, to questions of causation, then it is found, as the history of medicine abundantly illustrates, that on the one hand distinct diseases have superficial resemblances, bringing them to be confused, and that on the other hand conditions thought to be quite unconnected have proved to have a common pathological basis. Instances of both kinds of reshuffling will no doubt often occur again. As our understanding of disease is incomplete, so, and in that measure, our classification remains artificial and lacking in finality. There are, in fact, few clear-

cut discoveries relating to the mechanism of disease which have not called for some change of nomenclature; there are probably none that do not render close revision of parts of it advisable. While the progress of nomenclature directly and importantly affects investigation of mechanism, the latter often affects the former. There is an interplay between the two; they should move forward together; in investigation the nomenclature of disease, until based firmly on cause, should usually be regarded as unstable. Briefly, there are two points to realize. Firstly, that the useful nomenclature of disease is becoming less a matter of simple and casual observation, and more and more dependent upon a close understanding of the mechanism of disease, and therefore upon concentrated and intensive study. Secondly, that the attempt to reach accuracy of definition is itself a strong stimulus and powerful guide to investigational work.

Search for Cause.—There is no special method of investigating the cause of disease; knowledge of cause may be derived from one of many sources. The disease may prove to be hereditary; it may be acquired. The ultimate cause may become revealed after a characteristic group of symptoms is seen to be associated with some definite morbid change. It may be found to result from a specific bacterium or protozoon, from a fault in diet, from contagion, or from exposure to a poisonous environment. Causation has far more aspects than can be dealt with here. But one point of view peculiarly relevant to this discourse may be considered. Although it is manifest that the cause of disease may be found by bacteriological, protozoological, dietetic, or epidemiological study, it is also manifest that such studies depend for success upon relevant clinical discoveries. The cause of human disease never has been, and never can be, found purely within the walls of a laboratory; there must be at least some association, direct or indirect, with patients. This association has grown, especially in the case of bacteriology, to be too loose; it is quite necessary to the proper study of many diseases that the association should develop the intimacy which has proved so successful in investigation of tropical disease. Let us remember that Lister was his own bacteriologist. The solution is not usually to be found in what has been termed team work. Can it not be hoped that skilled clinicians will be found in this country who will devote their lives in studying the origin of disease, and who will fit themselves by special and appropriate training for this particular task; or is reliance still to be placed on casual contacts between pure

clinicians and specialists in laboratory technique or pathological method ?

Studying the Course of Disease.—Apart from those diseases which run short courses to death or to recovery, work of this kind can be undertaken satisfactorily only by those in long-continued contact with given patients; it is essentially work for the co-operation of the family doctor. Whether the course of the disease studied is modified by treatment or not, these investigations are fundamental to prognosis. Sound prognosis bases itself wholly and inevitably upon experience, a statement which needs emphasis. Much further information than we possess has still to be obtained, even in so far as relatively simple conditions of disease are concerned. Where close definition of disease is impossible, where in arriving at a prognosis several factors must be considered, the precise significance of the different factors has usually been insufficiently analysed. It is often work of a most tedious kind, and involves highly critical and sound statistical treatment. A most notable, I would say unique, example of such work is that carried out over a ten-year period upon 1,000 cases of chronic heart disease by Dr. Ronald Grant. His contribution will form the basis of sound prognosis in cases of this kind for very many years to come; I point to it for this reason, but especially because it is an illustration of scientific method in establishing prognosis.

Therapeutics.—The basis of all new treatment is, and always will be, critical experiment. Therapeutics is a purely experimental science, and its methods should, but unhappily do not, always correspond. There are a few drugs such as thyroxine and insulin, mercury and iron, digitoxin and morphine, the beneficial effects of which, when given to appropriate patients, are so rapid and so obvious that they are universally acclaimed as sovereign remedies. For the rest, a large proportion of the drugs in daily use are without scientific sanction, and no attempt has been made or is being made to obtain this sanction for them. There are many forms of treatment—electrical, balneological, and I would also add surgical—to which the last statement equally applies. Let me not be misunderstood. I do not mean that they are unsupported by a train of academic reasoning. There is in my view only one good reason why a remedy should continue to be used —namely, that it has been found to be of service; this standpoint is empirical, but that does not prevent it being fully scientific. Neither do I mean, in saying that many remedies are without

scientific sanction, that they are useless, but that there is no properly recorded evidence of their usefulness. One of the more obvious tasks of Clinical Science is to compile clean and readily available records of properly controlled observations proving the effects of such remedies. It seems unbelievable that the profession can long continue tolerant of a system under which drugs and other remedial measures are vaunted on an inadequate basis of recorded experience, or that doctors should be almost forced by popular clamour to give and to transfer allegiance, an allegiance stimulated by report or advertisement and not by proved worth, temporarily to this remedy or to that. It is not through easy, but often fallacious, general impressions that full progress is to be expected, but mainly through deliberate, unbiased, and untiring study of the reactions of patients to given remedies. It is the duty of Clinical Science, in this branch of its activity, to maintain close linkage with pharmacology; but it is equally its duty clearly to recognize that, in so far as both manner and intensity are concerned, the action of a drug on man is not necessarily the same as the action on an animal, and that the action on the diseased is not necessarily the same as on the healthy man. The proof or disproof of a drug's efficacy rests finally on the test in patients.

We have been dealing with methods in which the disease itself is the focal point of study; the isolation and recognition of entities, the inquiry into the cause, course, and treatment of disease. There are other important forms of investigation.

Study of Special Manifestations of Disease

A very profitable line of study is that which inquires into separate but various manifestations of disease, whether these are subjective or objective.

The study of subjective symptoms has a unique practical value in that it relates to manifestations directly affecting the patient's welfare; these manifestations of ill-health must be dealt with whether we know the cause of the disease producing them or not. But the study of subjective symptoms and other phenomena has a much wider importance, because it frequently throws a flood of light upon the processes and nature of disease, leading up to closer definition and to more intimate understanding. It was inquiry into the meaning of the symptom " palpitation " and to a closely related phenomenon common to many different diseases

—namely, irregular heart action—which led to the volume of productive work upon the mechanism of the heart beat. And it is to be particularly emphasized that that work comprised not only studies of the disordered heart beat, but of the normal heart beat as well. Since symptoms, using the word in its broad sense, may usually be regarded in terms of disturbed function, the study of symptoms necessitates very close consideration of the relevant mechanism of the body while this is working naturally. The study of symptoms, therefore, is the branch of Clinical Science which links it most closely with physiology and stimulates physiology to many of its most important new ventures. For the study of symptoms, when productive, always expands the corresponding branch of physiology. It is a notable fact that, in beginning to study a symptom, physiological information sufficiently complete to form a proper basis for the analysis of that symptom is very rarely available; often it is extraordinarily meagre. It must be sought in part or in whole before the symptoms can be studied. These preliminary studies fall usually to the clinician himself, for he alone is likely to realize the precise information required and also the importance of acquiring it. No one can work rationally upon the mechanism of symptoms who is not primarily a clinician, and who does not become in his chosen field a physiologist also. There is no branch of medicine, if we class disease anatomically, which does not profusely illustrate this fact. The work of neurologists and their fundamental contributions to our knowledge of the normal function of the central nervous system is no isolated instance, but it will spring at once to the minds of all.

Transient Phenomena.—It is frequent for a patient to complain of some symptom, or for some phenomenon to be observed in him, that is not continuous, but transient. Such phenomena cannot be studied too closely; they are most instructive and their investigation most fruitful. The transient phenomenon may consist originally of some relatively simple change, followed, maybe, by profound secondary disturbances. A very notable example of this kind is the paroxysm of tachycardia, a sudden increase in the rate of the heart beat to perhaps double, or more than double, its original level. These paroxysms come and go, and they allow us to study to perfection the effects of change in the rate of its beating upon the heart, normal or afflicted by disease. It is almost as though we were able to turn a rapid series of rhythmic shocks suddenly into the auricle and to study

the result at will; but it is actually done for us in the unanaesthetized subject, and without disturbance of chest wall or other structure. Sometimes the effects are profound. Within a brief period of the onset the subject presents the full signs of heart failure with congestion; a few hours later he presents none, the heart often returning at once to its normal size. In such cases the unique and recurring opportunity is presented of studying in its many aspects the manner in which heart failure develops. It is perhaps the most important experiment upon the heart which nature repeats before our eyes. You may study heart failure by comparing normal subjects with those who show failure. It is not the same thing; it is the chance of minutely comparing the normal and abnormal states of circulation *in one and the same subject* that is so important: here is the perfect control. You compare the one and the other state within a short time period; you can repeat and check your observations or modify your method of observation.

Provocation of Symptoms.—Symptoms are often transient, occurring under particular conditions that are related by the patient. From the standpoint of investigation it does not suffice to accept these statements; the patients should come under observation while the symptom is being provoked. In instances where the symptom is transient and oft recurring it is justifiable to ask the subject of it to undertake such an act as ordinarily provokes the symptom; but when this is done it must be under carefully controlled conditions, with a view to discovering any phenomena that are simultaneously displayed by the patient, and how these associated manifestations and the symptom itself are changed by varying the conditions of observation. There could be no better illustration of this highly valuable method of clinical work than recent observations upon intermittent claudication and angina pectoris. The two conditions have a closely allied pathology. For nearly a century and a half attacks of angina have been recorded; but it is only recently that we are really beginning to understand them. And this understanding comes from careful study of actual changes of pulse rate and blood pressure occurring during the attacks, and from studying by controlling them the factors involved in producing the attack, and correlating these with the corresponding observations on claudication. Many recent instances of productive work of this kind, enjoying as it does the full advantages of the experimental method, might be cited. It promises to develop into a fertile

branch of Clinical Science; for while bringing us into much closer contact with disturbed mechanisms, it has the power to unveil essential factors relating to the cause of the disease studied.

Systematic Observation.—This very important method of work has still a great future. In its simplest form it investigates all the circumstances in which a given phenomenon appears, and endeavours to find an invariable correlating factor. In the case of a symptom, such as headache, it leads to clearer definition of the immediate cause of that symptom. In other instances it displays unmistakably the effects of a manifestation, such as high blood pressure, common to several diseases. It may also teach how a given phenomenon is affected when circumstances vary spontaneously, then acquiring much of the power of the experimental method.

The Appeal to Lower Animals

In a brief survey it is unnecessary to do more than touch upon the obvious opportunities presented by studying the effects of simple injuries, especially of wounds. The historical example of the wound in St. Martin's stomach of which Surgeon Beaumont brilliantly took advantage is a sufficient illustration of its fertility. In the same connexion may be mentioned the equally fertile opportunities provided by studying the effects of certain surgical interventions, as in the well-known instance of the thyroid gland. I will not dwell upon the special opportunities for observation that present themselves to surgeons during the progress of operations; such investigations as those concerning referred pain largely depend on this method, which, however, must remain purely incidental, and must always be conducted within strictly prudent bounds. Deliberate interference, other than trivial interference or interference undertaken wholly in the interest of the patient, is not to be considered. It is for this reason of humanity that we appeal to the lower animals, and on this question there is still something to be said.

The use of animals for experiment is the chief, and rightly the most valued, method of the physiologist; it is equally indispensable to several branches of pathology and to pharmacology. But here the use of animals will be discussed from a very limited point of view—namely, its direct application to Clinical Science. A method of studying human disease, advocated since Claude Bernard's time, is first to reproduce such disease in a lower

animal, and then to proceed to investigate it in that animal. In theory ideal, in practice this is rarely possible of full accomplishment. Strictly speaking you cannot by cutting or tying operations reproduce any human disease other than one arising out of injury. It is possible to produce disease closely akin to that seen in man by introducing into animals the original agent of the human disease, bacterial or otherwise, or by withdrawing some essential from the diet; but, because the animal and its reactions are different, the disease is not accurately reproduced. The great value of animal experimentation in Clinical Science—putting on one side the indirect and great contributions of physiological experimentation—lies in a different direction. Disease destroys certain organs or interferes with the functions of certain structures, giving rise to specific manifestations; these *manifestations* of disease may often be reproduced in animals. If we can be sure of accurate reproduction, then their meaning and consequences in man may often be elucidated by further study. If by some interference we can occasion high blood pressure in a dog, we have not reproduced the disease "essential hypertension," and there may be no justification for supposing that the two states have anything in common beyond high pressure; we can learn much, however, of the effects of high blood pressure by this method. To entertain the belief that disease has been reproduced because one of its manifestations has been caused to appear is to fall into an easy but serious fallacy. A little thought will show the full significance of this statement and will bring the supreme importance of clinical research fully into view. When we investigate clinically some disease, or some manifestation of it, we may be seriously handicapped by circumstance; but we are possessed of the certain knowledge that what we study is precisely what we set out to study. That is a tremendous asset. The complete assurance is never enjoyed in studying an artificial phenomenon.

Status of Clinical Science

Clinical Science should lead the medical sciences of the future as it led in the past. It can do so by waking to full consciousness of its powers and responsibilities. There are many directions in which physiology and animal pathology are fundamental to the study of human pathology; Clinical Science, which includes human pathology, is fundamental to the proper pursuit of the

healing arts. In this direction it has direct powers of which the remaining medical sciences very rarely become possessed. Because of its immediate and constant bearings upon the health of the people it is the most responsible medical science. But it will be clear that if it is to take its rightful place in our community, then it must acquire full opportunities, so that it may be conducted, as are the other sciences, with the freedom of movement essential to vitality, being unhampered by collateral preoccupations and opportunisms. It must be conducted with that rigid adherence to truth that takes no heed of consequences. It must seek knowledge for its own sake, and be allowed to develop its own lines of progress. It must be judged, and its devotees must be rewarded, upon a new basis—namely, the weight of contribution to actual knowledge and to philosophic thought, without reference to the immediate practical result. This high status cannot be won for Clinical Science by the efforts of practising physicians and surgeons, however brilliant and unselfish their efforts may be; for these can be no more than occasional acts intermingled with very different activities, and the movement to be successful calls for concentrated purpose and sustained work over periods of many years. I am profoundly convinced that a movement such as is outlined is essential to the future safety of medical science, and am equally convinced that it can be accomplished without unusual difficulty by young and able men who can see the opportunities presented to their generation and are prepared to give themselves whole-heartedly to the fulfilment of this great purpose.

Briefly, Clinical Science to develop fully must become possessed of the traditional intellectual liberty of other sciences. It requires only that the simplicity and truthfulness of this ideal should be understood and its implications realized for Clinical Science to come soon into its own. Appointments to full-time service in medicine must be based neither on professional experience, nor upon capacity in teaching, nor upon simple erudition, but primarily upon distinction in the organization and accomplishment of work in Clinical Science. The focus of academic medicine must not be allowed to become the teaching of masses of students for professional objects; the focus must be the fostering of pure learning; and the main work of the day must consist of search and research, of the sifting of sound views from unsound views, of an impartial weighing and recording of evidence. Academic medicine must not be driven to a merely technical end-point, but

must assume the full dignities and responsibilities that belong to a great and progressive branch of knowledge.

There are questions which concern the elucidation of disease and its processes in man which are among the hardest that human intelligence and endeavour have to answer. Clinical Science especially requires, for the solution of its many and varied problems, men of first-rate ability. To the younger generation the opportunities now offering, the security that is assured, are of a kind undreamt of a few years ago. The days are passing when a worker was inevitably forced away from the work of his choice to find his livelihood in practice. The time is approaching very nearly to us when it will be impossible for men spending long years in the successful solution of problems in Clinical Science to fail to draw around them at least a few eager young workers from their own teaching institutions; when it will be impossible for them to remain isolated intellectually, and surrounded only by students preoccupied by more directly practical pursuits and interests.

Here and earlier it has been urged that the work of Clinical Science requires for its highest development men who devote their thoughts and labours to the investigation of human disease as a life-work. To bring into existence a group of such workers is the need, and in my view the opportunity, of to-day. But while impelled at the moment to this advocacy, I am not unmindful of the great work of practising physicians and surgeons in the past, or of the contributions of young able men whose ultimate destination is practice. Upon such work the growth of Clinical Science has till now depended and must continue largely to depend. The contention is that such work is not by itself adequate to keep Clinical Science abreast in the team of allied medical sciences.

The Recent Growth of Opportunity

Medicine, like the world generally, is passing through times of change, of critical readjustment to the spirit of our age. In the days before the war no man could pursue Clinical Science as his sole aim and live. There were a very few scholarships to help an occasional student to linger for a few years, but no more, before taking the plunge into practice. There were a very few sources from which small grants might be obtained to cover barely, or in part, expenses incurred in work. The first sign of

real change came from a generous and far-seeing private donor, Sir Otto Beit, who in the year 1909 endowed a Trust of nearly a quarter of a million pounds for the maintenance of a large group of three-year Fellowships. The Fellows first appointed had barely completed their term of work when the movement for the encouragement of research in medicine became greatly enhanced by the allotment of public funds under the control of the Medical Research Committee (now the Medical Research Council). This council has the power, and has repeatedly stated its eagerness, to establish units of clinical research in this country, paying adequate salaries and defraying all reasonable expenses. Since its inauguration it has gladly welcomed the chance to finance all reasonably promising schemes of clinical research submitted to it. Its influence upon medicine in this country has been an ever-growing one, not only because of the increasing importance and volume of the work done under its guidance, not only because its resources, financial and otherwise, have increased, but for a further reason which is perhaps the most important of all. Within a short time of its institution, and remarkably, it established in the minds of those working under it a feeling of security that they were free to pursue their work without other cares. Workers under this council have enjoyed an unusual measure of freedom from direction and from restraint, and a sense that both their present and their future interests are closely guarded for them. When the war was over the educational authorities began to take a part in this progressive movement, and there became established in London alone a number of professorial chairs in medicine, surgery, and gynaecology, with establishments that now provide full-time work for a much larger number of junior workers. Other benefactors have begun to found additional senior posts specifically intended for the prosecution of research in Clinical Science within the wards and laboratories of hospitals. One of the first of these was the permanent endowment by the Rockefeller Foundation of such a post at University College Hospital. It is being followed by the creation of a similar post by the Beit Memorial Trust. The Medical Research Council has established a neurological unit of a similar kind at the National Hospital, Queen Square.* I would ask you not to forget that two Foulerton professorships have been established by the Royal Society

* The Oxford Unit of Clinical Research has been established more recently.

in recent years, under a bequest " for original research in medicine for the discovery of disease, the causes of it, and the relief therefrom of human suffering," stipulations which clearly open these posts to purely clinical candidates, possessing appropriate qualities. The instances given are by no means exhaustive; but enough has been said to show how profoundly the position has changed within the space of a few years and how rapidly the movement is gathering force. It is most encouraging to see this College possessed of its own special research funds for the direct help of progressive work. The formation of a Science Committee within this College is another and important sign of a general trend of events and of the awareness of the College of the spirit of these times. Apart from their potential importance such innovations can hardly fail to form important and necessary links between the more purely academic and the practical activities of our profession. It would be gratuitous for me, in speaking before this College, to dwell upon the importance of such linkage. Every pathway between those who have new knowledge to impart and those who can apply it must be maintained; and though it is necessary that investigators should enjoy that measure of seclusion which is necessary to the full development of their powers, it is vital to our profession that there should be no sharp distinctions, no subdivisions into a camp of investigators and a camp of doctors.

But of all the developments happening in recent times none is so important as the lively awakening and steadily growing interest among young and able men in the clinical field. This change in spirit and in intention has now been in progress for a sufficient time for us to be assured that it is not transient, but has behind it a fixed and deep enthusiasm based on gathered courage and confidence, which now can hardly fail to carry the movement forward to ultimate success. The activity and fruitful work of many new recruits has enabled the formation of a new society,* now in the fourth year of its vigorous growth. The gatherings of the society, formed around a nucleus of full-time workers, but closely linked with the practising profession, are already notable for the original quality of the communications which it welcomes and informally but thoroughly discusses. Simultaneously, among those men, and they are now many, who are steadily pressing this movement forward, there has developed

---

* The Medical Research Society.

a spirit of mutual interest, help, and encouragement, founded upon a common purpose in work; there has come to clinical workers in the great schools a far closer community of feeling and of sympathy, not only with each other, but with workers in physiological, pathological, and other fields of scientific endeavour, connecting links of the utmost promise for the future. Clinical workers are beginning to acquire their share of that subtle atmosphere which is common to university life, and is well recognized to exercise profound influences upon the spirit and method of work and of thought.

Concluding Remarks

A little more than three hundred years ago the clinician Harvey, inspired by the desire to understand the motion of the heart, set himself to discover its meaning. He did not concentrate upon such of its motions as he might see in his patients; but starting from these, in his wisdom, let his gaze roam. He searched widely, gathering with his own hands and with his own brain all such information as he might find pertinent to the furtherance of his studies. It was because he possessed breadth of vision that he brought these studies to a conclusion that has proved fundamental to all medical science. In doing so he gave us an exposition of method of unique force and significance. He established for us a tradition, a tradition that Clinical Science shall not be confined narrowly or by artificial boundaries, but shall be free to search how and where it will: a tradition which will not countenance attempts to separate the study of health from that of disease, or the study of animals from that of man, or work at the bedside from that of laboratories. These are among the lessons of *De Motu Cordis*, and this is Harvey's leadership. As your chosen Orator to his memory to-day I recall to you as Fellows and Members of this College his exhortation to search and study out the secrets of nature by way of experiment. I am moved to ask here and in his name that this College should allow its gaze to rest with earnest pride on this gift of Harvey's, the Clinical Science which he founded; that, visualizing the special needs of this science under the strain of modern conditions, the College may offer full encouragement and sympathy to its appropriate development: I ask that it should couple Harvey's name particularly with this science, so that it may stand to him in this College for his

most lasting monument, and that it may form a bond within this College between those who live to gather knowledge and those who live to apply it to the needs of the sick, a bond bringing mutual affection and respect.

To you, Mr. President, I desire to express my humble thanks that you have afforded me opportunity of promulgating these ideas, at this time, in this place, and on this occasion. For in respect of time, the signs of its ripeness are abundant; in respect of place, this great College has for centuries formed a chief centre, not only within Great Britain, but in the world at large, of progressive clinical thought; and in respect of occasion, we are here to offer tribute to the most illustrious son of this house, to him who gave us his noble example of the Science of a Clinician.

THE HUXLEY LECTURE ON CLINICAL SCIENCE WITHIN THE UNIVERSITY*

I

In response to the invitation with which you have honoured me, I am going to plead before you to-day that the biological science in which I am interested should be given fuller university status. It is a special privilege to be allowed to bring these views to a university closely engaged in considering problems of medical education, and in a lecture commemorating a man universally recognized as a chief pioneer both in science and in education. But though Thomas Huxley gave his brilliant mind to the search for and diffusion of knowledge in physiology and in other branches of biological science, his interest was not directed to studies of human disease. For this reason you will perhaps permit me to turn for a moment to his distinguished contemporary, James Paget, who, at first similarly attracted by biology, was subsequently drawn on to become a noted investigator of disease in man and a most gifted surgeon. It was the development of Paget in this direction which enabled him, while very conversant with contemporary biological and physiological thought and fully alive to its importance, to make these deliberate and weighty statements in speaking of clinical work:

"I feel sure that Clinical Science has as good a claim to the name and rights and self-subsistence of a science as any other department of biology; and that in it are the safest and best means of increasing the knowledge of diseases and their treatment. . . . Receiving thankfully all the help that physiology or chemistry or any other sciences more advanced than our own can give us, and pursuing all our studies with the precision and circumspection that we may best learn from them, let us still hold that, within our range of study, that alone is true which is proved clinically, and that which is clinically proved needs no other evidence."

---

* Delivered at the University of Birmingham, March 14th, 1935, and first published in the *British Medical Journal*, March 30th, 1935. This address includes a reply to the Presidential Address of the Royal Society (*Proc. R.S.*, B. 116, 403), which address criticized my previously expressed views.

I use these clear and memorable sentences of sixty-five years ago, with which I find myself thinking in such close accord to-day, to introduce the matter of the present address. In using the term "Clinical Science" Paget thought exclusively of researches upon living men. I shall use this same term to convey his meaning, but shall widen it, and define Clinical Science as the branch of knowledge that centres upon diseased human beings, but which also includes directly relevant parts of the allied sciences. The central field of work is the human patient; and this will remain the field peculiar to the science. But work will often extend to more distant boundaries; these will include human physiology, because the normal is the control to the diseased man; will include the morbid anatomy and the bacteriology of man; and will include "experimental medicine," which has come especially to mean such experiment upon the lower animals as is inspired by clinical problems.

I am putting before you the idea that Clinical Science *so defined* should find its place and acquire proper status within the university. It has sometimes been pointed out that what I have spoken of as Clinical Science is not clinical medicine and clinical surgery as these are practised, and that certain of its activities might justly be termed physiology or pathology. I know that this is so. The distinction between the science and the art of medicine is intentional; they should be kept as separate ideas, and regarded as separate directions of human endeavour, in working to practical ends; though it is to be said that there is no wish to infer that the science and the art can never be pursued by one person. By intention, too, distant boundaries have been fixed. To give independent strength to the science it must be unhampered in its movements. The boundaries between individual sciences may not be maintained rigidly without hindering progress; overlaps between Clinical Science, pathology, and physiology are vital to medical science as a whole; they bind together three activities, which in process of time have grown too far apart from one another. We are here engaged in discussing a serious practical question—namely, the relation of the university to branches of knowledge that concern the health of mankind. The university cannot successfully enter this field of activity without setting up within its own domain a strong scientific department inspired by direct clinical interests.

In attempting to picture Clinical Science within the university,

the relation and attitude of the several medical sciences to each other obtrude themselves. In recent times discussion has sometimes turned from mere statements of achievement, possible and real, to a comparison of the merits of these sciences, or to a comparison of the relative importance of the contributions from ward and laboratory. These two comparisons are of course distinct, for in many of its activities Clinical Science is to-day essentially a laboratory science. I understand by a laboratory a place well equipped with apparatus and other facilities for working by special and quantitative methods; I cannot accept the common notion that a laboratory is a place to which animals but no patients are admitted. It is proper and necessary periodically to describe and emphasize the mass of knowledge that mankind undoubtedly derives both from the physiological laboratory and from the clinical ward with its associated laboratories. But an attempt to assess the relative merits of these different activities swings discussion from useful into controversial channels, and simultaneously diverts attention from central truths—namely, that the merits of these various forms of work are not in fact comparable, and that both Clinical Science and physiology, both ward work and laboratory work, are absolutely and therefore equally essential to the attainment of those practical goals towards which all groups of workers in these fields are striving. The sciences considered form a closely interconnected series, which should be encouraged to grow as one harmonious whole. To the more applied science, any growth in the purer science that nourishes it is a gathering of its own ultimate strength; and any growth in the applied science promises to the work of the purer science a surer fruition. The welfare of each member of the series is highly important to all other members; the general strength will largely find its measure in that of the weakest component. Here, therefore, is the strongest motive for mutual good will and support. Nothing will more quickly bring the allied medical sciences into close understanding and sympathy, which from every standpoint is so desirable, than that they should be placed upon just the same footing within our universities; it is essential to this purpose that each should be founded on the cardinal principles, which shape sciences universally, and that each should possess scope sufficing to give independent strength.

II

It is necessary to my main theme to show that Clinical Science has the power to contribute abundantly to progress, and this leads me on to speak briefly of the broad relations of this science to problems of human disease. Studies of disease cannot begin until disease is recognized and in some measure understood; real knowledge of disease is derived only from disease itself, it is not built up in the form of concepts of perverted function. No human disease is known to be curable until man has been cured of it. It is in the very nature of things that, in its initiation and in its culmination, work relating to human disease must be done on man himself. Once the problem has been formulated, and before the final work on man is done, in a middle phase, study may become diverted to the lower animals. Such work, provided that it still concentrates upon the problem of disease, is still within the province of Clinical Science, as this has been defined, though, be it granted, it would fall without under a narrower definition. But under one or other definition the facts here briefly stated bring us to comprehend that the opportunities of Clinical Science in guiding and co-ordinating work as a whole are unique.

Notable advances in the practice of medicine and surgery come chiefly out of work concentrating upon the main problem studied in its natural environment, rather than from researches of a more remote and recondite kind. These latter are not to be placed upon one side as unimportant, or even as less important, to progress: but their influence is less immediate; they provide us with methods of precision; they suggest to us tests to employ and experiments to undertake; they help us much in explaining and appreciating the manifestations of disease and the mode of action of our remedies. But the management of disease is rarely to be deduced from first principles; it is the direct approach that attains swift success. The value of facing the problem directly has been illustrated repeatedly. When Pasteur was called to investigate fermentation in wine, the disease " pébrine " in silkworms, and the disease anthrax in sheep, he dealt with each of these problems in its stronghold; starting from the disturbance or the disease itself, he concentrated upon it and ultimately and in each case himself brilliantly won the remedy. Jenner similarly dealt with small-pox, seizing

during his clinical experience of the disease upon folklore, from which he worked on and subdued a scourge; in like fashion and starting from compound fractures Lister attacked and swept away the sepsis of surgery. It is not often that the efforts of one man can achieve so much: but the same form of successful attack can be illustrated by the work of Ross upon malaria, of Mackenzie upon auricular fibrillation, and by the recent work of Laidlaw and his colleagues upon the distemper of dogs. A more involved illustration is that of the discovery of insulin and its effect in diabetes mellitus. First comes the recognition of the disease, then recondite laboratory research happens upon a new effect of excision of the pancreas, both essential steps; but the present aptness of the illustration lies in its final stages. Banting, a young doctor familiar with the malady diabetes, was inspired by learning from a clinical paper that obstruction of the duct of the pancreas leads to atrophy of the excretory parts of the gland; out of this idea he directly devised the method that first proved the presence of an internal secretion, insulin, in the remainder of the gland, and from this Banting and his colleagues themselves drove on to the end-point, the successful administration of insulin to patients.

These illustrations, by showing how important to the result is concentrated attack upon a problem in its own environment, place Clinical Science and the more recondite sciences in their true perspective. I pass on to consider some ways in which Clinical Science works.

Scientific discovery begins with observation, and it will clearly be perceived that discovery relating to disease forms no exception, but will always start from observations upon the disease. On previous occasions I have emphasized the importance of new descriptions of states of disease, and the associated attempts to define them precisely, knowing how vital is this branch of our work to progressive study of disease. Those who come into intimate contact with clinical researches are soon forced to recognize the profound influence of terms on progress, and the value of their accuracy. When we name disease, we invent terms which we realize to be abstractions. They represent abstract conceptions in precisely the same sense as do the terms atom, cell, vitamin; and as terms they serve exactly similar purposes.

A physiologist begins an analysis, he isolates what he terms carbohydrate, fat, protein; he attempts to study the origin,

nature, and destination of things for which these terms are supposed to stand, and, as he studies, new qualities are found, he subdivides, and new names and new definitions spring up. So with our named diseases; we set out to isolate and to bring sharpness to our image of states of disease and begin to inquire what they are and whence they come. The names of diseases are not immutable. As with the physiologist, so with us, our terms change and our definitions shift as we work; such changes are inevitable, and constitute the adjustments, nay a measure, of progress. Our terms stand to us for conceptions which may be relatively vague or relatively precise, but which serve for intercommunication, and are numbered, like working hypotheses, among indispensable tools. Our definitions are beams of thought focusing in terms, and this focusing and sharpening of focus represents a high form of intellectual endeavour. But these mental activities, conveniently summarized in terms and in definitions, depend upon the close and endless work of observation and experiment, upon inquiries into the manifestations of disease and into its origin and natural history. This kind of work is little understood outside the ranks of actual workers in this field. There are the early attempts to isolate and separate states of disease in patients upon the simple basis of observation in the living. The resulting subdivision is crude, and constitutes no more than a beginning. There follow the more intense refining processes; hypothetically separate states of disease are closely scrutinized, underlying morbid lesions are examined, the various manifestations of the disease are subjected to analysis, the cause of the disease is explored, and found perhaps in a change of environment or in the introduction or growth of organisms within the body. In these studies especially, knowledge and method derived from the allied sciences are used, in these studies the observational method begins to be found inadequate and the experimental method comes into play. The studies are intricate and often highly technical, and, as they proceed and the nature of the malady and its natural history are explored, ideas alter, and we begin to define and to rename. Thus, while the early names base themselves on simple observation, the later names and definitions base themselves on far more elaborate and difficult studies. The simple process of observation, from the nature of the case, soon tends to exhaust itself, but the accurate defining of states of disease does not. Here is an ever-present opportunity of Clinical Science. The

defining of states of disease, as it has been outlined, is fundamental to and inextricably interwoven with the study of these diseases; I affirm that this defining is yet in its infancy, that its primitive state is inhibiting the progress of medical science, but that as this branch of knowledge develops, our conceptions of states of disease will change profoundly.

There is more to be said about experiment; for in this direction our point of view in studying disease has begun to alter rapidly in recent years. It should be manifest that whenever we interfere with a patient, as when we employ any test of a patient's reaction to something introduced from without, to exercise, to a foodstuff or to any other substance, or to any physical stimulus, we experiment. Experiment has long been applied in the sphere of treatment. Every remedy employed to-day is the result of experimentation on man, every new remedy we shall employ in the future must inevitably pass through this experimental stage. The method has made three incomparable gifts to medical science and the welfare of mankind —namely, vaccination, antiseptic surgery, and general anaesthesia. Experiments judiciously selected and properly conducted are not only harmless, they are often or usually beneficial, teaching us more rationally to regard and manage the patient's condition. Those who have charge of patients are in this respect loaded with a grave responsibility; only harmless or beneficial experiments are justifiable. It must be understood that responsibility cannot be evaded by refusal to experiment; experiments always have been conducted, always will be, for they are essential to progress. It is obviously very important that such experiments as are made should be made with the consent of the patients, should be made in a safe manner, and that they should be conducted so that they give reliable answers to the questions asked. Of experiment in medicine, and especially in the examination of remedies, much that is done lacks precision and proper control: consequently, much of the work is valueless and must be repeated, and so very many more experiments are carried out than is necessary to determine the results. You will apprehend that, in so far as remedies are concerned, I am advocating a reduction in the numbers of experiments performed, and their conduction upon a stricter, safer, and more productive basis than hitherto. But the fruitful application of the experimental method to states of disease does not end with an investigation of remedies. There are many opportunities of

testing patients by harmless interferences in such ways as to explain manifestations and causes of disease. In this direction the experimental method has only begun to bring us new knowledge. That is so because, apart from the search for remedies, more purely observational methods are traditionally natural to practitioners of medicine and surgery. The experimental method requires for its employment a special outlook and training; thus, those having easy access to patients use this method and its controls little; and those trained to employ it are often denied the access which they desire. Those working in other fields are able to recognize directions in which the method has already achieved success; but only those intimately in contact with the work of Clinical Science perceive the manifold directions in which it remains to be applied. I will not weary you by enumerating past results, or by pointing out specific problems now requiring this method of study, but will state categorically that it is peculiarly appropriate to innumerable manifestations of disease, and to many problems of causation. This statement is true not simply of one or two types of disease, but of problems connected with almost every variety of disease; the method is of almost boundless application. Experiences of work in diverse directions convince me that experiment is as fundamental in investigating disease in patients as it is in the case of other biological sciences, and that Clinical Science properly established would soon demonstrate the fact abundantly.

This general survey of the particular activities of Clinical Science, activities which have been considered previously on more than one occasion, will suffice to show that this science deserves very full encouragement. Briefly, the starting-point of the practical problems of medicine is the patient; it is clinical work alone which can formulate the problem, and it is the clinician who is best placed to guide it to a fitting termination; but this directing of work requires wide knowledge and often intimate experience not only of clinical methods, but of the methods and facilities afforded by allied sciences. The experimental method, which I foresee must play an increasing part in future work, calls for a particular point of view acquired by long training and experience in suitable original work. The employment of intricate apparatus and intimate knowledge of tests of precision make deep inroads into reserves of energy and time. While we may still expect to see many valuable dis-

coveries coming from men engaged in the practice of medicine and surgery, it is increasingly clear that in many directions advances would more readily come from men free to give their undivided energies and thoughts to the work of progress, to the exclusion of routine. It is for this reason that the establishment of posts of clinical research has seemed so necessary. Because men must be trained to do such work, because it is right that those professing such work should enjoy the stimulus derived from teaching and from an appropriate atmosphere, it is necessary for Clinical Science to become established within the walls of the university.

III

To discuss the important matter of educating medical students is not the object of this lecture; but in ascertaining the place of Clinical Science within the university this problem necessarily comes under review. It will be common ground, whether knowledge to be conveyed refers to practical art or to science, that education is most vigorously conducted by close contact between the student and the master of his subject; methods of work are learned from demonstration, methods of thought by conversation; communion inspires. If you would learn to do a thing, you go to one who does it well; you watch, you listen, and your own first attempts are made under supervision. The teaching of medical practice began and has continued until recent times under such a simple system of apprenticeship. The final stage of medical education in this country still rests solidly upon this traditional foundation, centring upon clinical demonstrations and upon a system of clerkships and dresserships; the high value of this method has obtained wide recognition. The most fitted to teach practical medicine and surgery are those successfully and daily engaged in its practice. But in a simple apprenticeship system, where a doctor has but one or more pupils, the standard of teaching would vary too greatly and would be in the average too low; therefore students are brought together under teachers chosen for their unusual energy and ability. While it is obvious that the best teacher for general practice is a first-rate general practitioner, such practitioners could rarely engage actively in teaching numbers of students and yet pursue their ordinary work. For the reason that among able doctors hospital physicians and surgeons are the most readily available, teaching has fallen into their hands. This has formed for many years

a more or less satisfactory compromise, though one that becomes less so in the measure that consulting work grows more specialistic in its outlook. The main principle emerging is that the final stages in the preparation for practice should be conducted by practising physicians and surgeons. Efficient medical practitioners are not scientists; the bulk of their daily work is not to create or to discover, but to follow the established rules of an art; as in the case of other practical arts their work, while highly skilled and often ingenious, is nevertheless chiefly repetitive. Preliminary education in science I shall allude to later, for the moment it is the immediate preparation for practice that concerns us; of this I would say, that as it is an art, it should be taught by those who practise it; and we may be content that while such a system is basically sound, it happens also to be the most economical.

Enough has been said to enable me to bring the final stages of the education of the medical practitioner into proper perspective and to display more clearly my views of the main functions of the university in its relation to the final subject of the medical curriculum. In thinking of the education of the practitioner, one is forced to wonder if the final stages of his preparation for practice form an appropriate activity for the university. But this question of the extent to which it is proper for universities to prepare students along technical lines is one affecting so many other forms of vocational teaching that I shall not venture further to consider it. I shall assume for the purposes of present argument that universities will continue at least to direct vocational education. But if this assumption is a correct one it is necessary to come to two conclusions. On the one hand, the university must be prepared to train, or to direct the training of, students in medicine as a practical art. On the other hand, it must take heed that this vocational training does not displace, does not prevent the full development of, the purely scientific pursuits of medicine within the university.

It would seem quite unsound from the university standpoint that the training of either practitioners or consultants should be regarded as a primary or direct function of a professor of medicine or of surgery. I do not mean that it may not be his function largely to control such training; I do mean that the transference of any of that work of training medical practitioners, which has been undertaken so well in the past by practising physicians and surgeons, to the professor would be

a fundamental mistake. This training is manifestly the work of practitioners; it is very definitely not the work of professors in the full university sense.

The man who is fit to train the practitioner to begin his life-work must be thoroughly familiar with everything that makes for successful practice, must himself be in practice, must not only understand disease and its management in its detail, but must be competent in managing sick people and their friends; he must be familiar with the science, skilful and precise in the craft, and versatile and diplomatic in the art. But these varied accomplishments will not fit him for the professorial chair. The work of the professor as I view it, though from many points of view intimately related, is distinct from this. I ask the questions: Is medicine in its final aspects to be studied only by those engaged actively in its practice; is there to be no study of general concepts and of method, such as would be in more obvious accord with the recognized standard of university departments of science? Are we to remain content that activities of the latter kind should be pursued within the walls of the university solely in the departments of anatomy, physiology, and pathology? To answer these questions affirmatively is to deny medicine real university status; to answer them negatively is to accord that status.

To make clear what I have in mind I will attempt broadly to sketch professorial duties. The first duty of a university professor is the advancement of knowledge of his subject by research; his second duty is to encourage and to disseminate learning. These are the accepted duties of professors in scientific departments in all universities worthy of the name; there is no reason to view differently the duties of those who profess the science now under discussion. The professor should be chosen primarily for his distinction in his science, or for his promise of such distinction. I see no reason why distinction as a practising doctor should be regarded as a qualification, but reasons against. There is no reason why the professor should not be appointed, as often are his colleagues in other scientific departments, as a young man. He should have been engaged actively in research in an appropriate field—namely, work that is actually undertaken upon patients, or laboratory work that is directly related to, and inspired by, problems arising in the ward or out-patient department. Such work he should continue, and he should supplement it by personally directing the similar activities of

his pupils. His own research work will probably develop, as prolific work nearly always does, along special lines, but a broad outlook will be maintained by his contacts with the work of colleagues and assistants, and by his teaching.

His teaching should have breadth, and in its outlines should be accessible to all. I have already indicated strong dissent from the idea that it should be concerned primarily with the needs of practice. It should deal chiefly with the principles of the science, and with patients only to exemplify specific points.

The professor should study and expound the history of the medical sciences, and especially of Clinical Science, and this should be done, not from the biographical standpoint, but with the definite motive of determining the ways in which problems are solved. His business is not with the eccentricities of medieval quacks, but with the origins and development of ideas in the minds of chief contributors to medical knowledge; and here, because true and full information is more readily available, his search should relate mainly to recent rather than to remote discoveries. He will trace the growth of knowledge, and will examine the influences of those systems or schools of thought which have prevailed. All this he will do in the attempt to discover sources of knowledge, prolific and sound methods of work.

He should deal with the problem of the cause of disease, or the conditions engendering disease, in its very many aspects; his survey will cover very wide fields; he will discuss the principles of hereditary transmission as these are known to relate to human disease, and he will inquire into the broad questions of the reaction of man to his environment; thus he will discuss on general lines such problems as the meaning and effects of infection, the significance of sexual, racial, and other predispositions to disease, and the influence of one state of ill-health upon another, the reaction of human tissues to physical injuries and to simple chemical poisons, the meaning of curious susceptibilities to more complex poisons, and the manner in which new growths arise in man.

He will attempt to analyse outstanding manifestations of disease, such objective phenomena as arise out of altered metabolism, changed temperature regulation, and water balance, and such subjective phenomena as pain, breathlessness, exhaustion, and the like. I have in mind the consideration of these and similar manifestations both upon broad lines and in detail,

but always with special reference to human disease. There are innumerable matters of this kind for profitable discussion and work; they should not be relegated to the pathological laboratory or theatre, for this procedure breaks contact with the human material, which can be used so aptly to illustrate, which inspires related laboratory problems, and itself so often constitutes the most prolific focus of inquiry.

In regard to diagnosis, prognosis, and treatment let these be dealt with again on general lines, by the laying down of guiding principles, rather than by the systematic consideration of specific diseases. It would be well to ensure first of all that what is meant by diagnosis should be understood and to contrast it with new descriptions of states of diseases; to explore error in diagnosis and its practical significance, and to emphasize the need for its complete avoidance in research that depends on diagnosis; and to compare the empirical method of diagnosis with that based on understanding. It would be well to show the proper basis upon which prognosis is built up, and to discuss the relative values of theoretical and empirical methods of procedure. It would be well to explore the ways in which new remedies are discovered; to show how the curative properties of remedies or their power to alleviate are to be ascertained; and to teach how, by research, the use of known remedies may be rationalized, the remedies perfected, and the field of their efficacy widened.

In all these directions the professor should strive to formulate and inculcate the principles underlying methods of work and thought in his science; he should insist upon the constant need of accuracy in observation, and of controlled experiment; he should teach the student to appreciate the value of evidence, and encourage logical forms of argument and of sequential reasoning. He should stimulate inquiry. He should display the details of his own researches and those of others, and illustrate discovery and the principles laid down, by repeating chosen observations and experiments in the form of demonstrations.

There is no branch of medical science that offers more opportunity for demonstrations, there is none which offers more occasion for the use of reasoned argument, there is none which calls for higher intellectual attainment; for there is none which combines so many forms of scientific activity, and none in which the worker is forced so closely and so frequently to consider both simple and complex problems of cause and effect. It is in his physiological studies that the medical student is first called upon

to stretch those intellectual faculties which are particularly used by scientists; for this reason physiology is rightly recognized to possess great value as a mental discipline. But this beginning does not suffice. These faculties should be stretched farther and kept under tension. Not only has Clinical Science the fullest competence to effect this development, but, so far as medical men are concerned, it is manifestly the most appropriate branch of science in which to bring these mental faculties to their highest efficiency; for it is the branch of science with which medical men will remain in closest contact, or that in which they will actually engage, not for a few years, but for the rest of their lives.

It is not for me to present you with a syllabus, but it has been incumbent upon me to show that there is ample scope in the shape of research, lectures, and demonstrations within the field of Clinical Science, for professorial endeavour, apart altogether from a systematic consideration of the diagnosis and treatment of diseases. These activities, supplemented by the work of guiding others and efforts to maintain firm links with the allied sciences, are enough, and with well-defined facilities can be brought to successful accomplishment.

The first requirement of a university department of Clinical Science is an out-patient and in-patient service sufficient to provide ample material both for the research and the teaching in the department. There is room for difference of opinion as to how large such departments should be, especially as to how many beds are necessary; this will decide itself partly by the local conditions and by the particular activities of the professor and his assistants. The first point I would make is that the cases upon which work is being done should be under the full control of the worker; experience has shown that this policy is alone practicable. Productive clinical research usually comes from an intense study of carefully selected cases, rather than from less careful studies of larger numbers; certainly this is so in all but the preliminary stages of investigation—that is to say, once the problem has become defined. Consequently, though large numbers of beds under the control of the worker are not required, the professor and each of his research staff must have an adequate complement. I regard the ideal arrangement as one in which the worker, while fully controlling, and having full responsibility for, all cases under investigation, is further provided with a service of patients from which he can select, and

that he should be relieved from responsibility in the management of these; such relief can be given in a number of ways, as by clinical assistants drawn from those concentrating upon practical medicine, or by the close linkage between professorial and other services. Some additional beds will be required to satisfy the needs of teaching; but the total number required is definitely less than ordinarily serves a teaching unit of practical medicine or surgery. Out-patient and in-patient service should balance; and the system adopted should be adjustable so that the professor or his assistant may be free to concentrate from time to time on cases in one or other service, or upon work on lower animals.

Adequate laboratory accommodation is a second requirement; here the need, in so far as space, fittings, and equipment are concerned, will be appropriate to the type of work, and will vary from time to time. It is important to the workers that these laboratories should be as close to the clinical services as possible; in part they may be directly attached to ward or out-patient department; sometimes it can be arranged that they will serve for the special and uninterrupted examination of either patient or animal.

IV

In considering the usual medical curriculum of to-day, and asking where in it Clinical Science is to play its part, I would start from the statement that this curriculum is already overloaded. So far as the majority of medical students is concerned it is overloaded by the preliminary sciences, including anatomy, physiology, and general pathology; so far as all students are concerned it is overloaded on the clinical side in directions presently to be indicated. My personal belief, founded upon over twenty years of constant teaching of medical students, is that most medical students derive less intellectually from their preliminary work than is often supposed. But I have also become convinced that while for the more scientifically minded student very full measures of physics, chemistry, and physiology are invaluable, the final work of the hospitals, as now conducted, is far from satisfactory. It is unsafe for any man, whose training in science has started well, to accept, on an authoritative and not on an evidential basis, the mass of statement of fact and hypothesis that the final student is now called upon to accept. There is, too, the very grave objection that during several years out of

those most important to a man's intellectual development he is occupied in amassing disconnected information to the exclusion of contemplation. It seems beyond reasonable denial that the full course of work, supplemented as this is by purely clinical appointments, if suited to the training of a practitioner, must be unsuited to the man who is to make science a career; it is wrong that he should so long be denied the chance of engaging in, or of being at least in close contact with, original work.

I think it is agreed that there are grave defects in medical education to-day, but agreement as to the remedy would come nearer if it were recognized that the right methods of educating practitioner and scientist are not the same. Cautious exploration may be necessary. But there is the principle to guide us—namely, a measure of separate consideration for two classes of student. I suggest as a basis of discussion the following plan in broad outline, and to simplify I shall omit considerations of those sciences which by consent form part of the basis of a general education.

There should be a first course consisting of outlines of human anatomy and physiology, both taught to ensure a clear understanding of the main mechanisms of the body and with special stress upon the needs of the final qualifying courses of instruction—that is to say, it should be particularly emphatic in teaching what is immediately applicable to the study of disease; for such matter will always command particular interest, and will be the more willingly and more easily remembered. It is not suggested that this course should consist exclusively of such material, for much must also be taught solely to illustrate the principles of the sciences concerned and their methods of work.

The second course should be one of more advanced physiology, suited to a science degree and to those who intend to take up physiology, pathology, or Clinical Science as a career. It should not be compulsory for medical students; it would ordinarily be taken in direct sequence to the first course, in some cases it would culminate in an honours degree, in some instances it might be delayed.

The third course should be the final or qualifying course, and it should consist on the one hand of general and practical instruction along present lines. For the qualifying examination it is necessary to demand a general standard of professional knowledge fitting the graduate to begin independent management of patients; but present-day teaching must be simplified

in all its branches. What is essential to general practice should be retained and emphasized, but there should be ruthless pruning away of what is insecure, intricate, or redundant, and of what belongs peculiarly to the province of specialism. Teaching should mainly concern itself with common maladies and with remedies of proved worth and ready application. There should be a determined effort to arrange facts and ideas in thoughtful sequence, and to train students to think logically as well as to observe keenly. This simplified final course should include the outlines of Clinical Science, and, thus arranged, should be the regular course for all medical students. Higher examinations should, I think, definitely differentiate between two classes of students. The Royal Colleges and perhaps the universities would still hold advanced examinations primarily testing professional abilities. But there is room for a university degree in medical science, which should not include medicine, surgery, or other branch of medical practice as such, but should centre upon disease, as this is studied in human beings; and this degree should be intended to mark those contemplating an academic career.

It is my conclusion that it is largely within the power of our universities to establish the branch of work that studies disease in living people as a science, by removing the obligation to engage in and teach the practical art, and by treating Clinical Science on precisely the same basis as the allied sciences, physiology and pathology, are treated, thus bringing all the work into real harmony. The maintenance of equilibrium among these three may then be left with confidence to the interplay of individual and general needs, and to the mutual sympathy that is natural in any corporation of enlightened people working to a common end.

Printed in Great Britain by
Billing and Sons Ltd., Guildford and Esher